



# **MIND**

**CONTENTS OF VOLUME LXXI**



# MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY

EDITED BY

PROF GILBERT RYLE,

WITH THE CO OPERATION OF PROF SIR F C BARTLETT AND  
PROF C D BROAD

NEW SERIES

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# M I N D

## A QUARTERLY REVIEW

### OF

## PSYCHOLOGY AND PHILOSOPHY



### I.—THE OPEN FUTURE

BY BERNARD MAYO

How should we characterise the difference between future-tensed statements and present- and past-tensed ones? Or is there no difference except the commonplace ones (1) that the events described stand in different temporal relations with the event which is the utterance of the sentence in question, (2) that the kinds of evidence available are generally different and have different degrees of reliability?

But if these are the only differences there are, why is there always something rather uncanny about doctrines that emphatically assimilate future to past · precognition, reversed causation, fatalism and the like?

On this last point, it may be enough to show (as Ayer does in *The Problem of Knowledge*) that discomfort about these doctrines rests on misunderstandings. Fatalism, for instance, appears to deny the obvious fact that, while we cannot in the least choose what the past shall have been, we can to a considerable extent choose what the future shall be. Yet the fatalist maxim "What will be, will be", and even the stronger version "What will be, must be", and "What now is, must needs have been", are all tautologies and therefore cannot even tell us anything of interest, let alone anything uncanny. The "must" in these maxims is the "must" of logical necessity: if something is going to happen, then it necessarily follows that it is going to happen, if it is now happening, then it necessarily follows that it was going to happen and will have happened; if it did happen, it necessarily follows that it had been going to happen and has happened. In so far, it is perfectly true that we can no more change the future than we can change the past, we cannot choose that the future be different from what in fact it will be, any more than we can choose that the past be different from what in fact it was. But—and

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here Fatalism goes wrong—we can choose that the future be different from what it will be *if we do not choose*

Similarly we *were* able to choose that the past be different from what it would have been *if we had not chosen*. But this similarity is not the complementarity required to substantiate the thesis that “in logic” the past is just as much, or as little, “open” as the future. For while it is the case that we can here and now choose that the future be different from what it will be if we do not choose, it is not the case that we can here and now choose that the past be different from what it would have been if we had not chosen, either now or at any other time. Ayer is, indeed, forced to admit that there is, after all, a sense in which the future is open and the past is closed—namely that, “whereas our present actions can have no effect upon the past, they can have an effect upon the future” (p. 170).

As a means of characterising the difference between past and future, however, this is open to the *prima facie* objection that it is the inefficacy of our present actions on the past which depends on the pastness of the past, and not the other way round. One may easily insist that the difference between future-tensed statements and past-tensed ones is that one sort refers to the future and the other sort to the past, and that this difference remains to be explained. Ayer’s final answer, indeed, is not in terms of cause and effect; he holds that the only relevant difference is a difference in the extent of our knowledge. We know very little about the future, but we know rather more about the past. But this is only a difference of degree—the commonplace distinction (2). It cannot be made at all precise, since there are large local variations in the relative degrees of our knowledge (some future events are better known than some past ones), and there is the suspicion that the cart is before the horse again, that it is the futurity of the future that curtails our knowledge.

Why do Ayer and others look to agent-centred criteria—to questions about what *we* can do or know? Is there an *a priori* reason why Nature should not be consulted? It seems that there is. For it is easy to agree that no events are, as such, past present or future; these are relational predicates and the other term of the relation is the event constituted by the utterance of a sentence or the occurrence of a thought. But sentences and thoughts require speakers and thinkers; therefore no elucidation of time-concepts is possible without reference to persons.

“But this is a mistake.” It is perfectly conceivable that natural processes should exhibit characteristic non-symmetries, so that, given a certain phase of such a process occurring at  $t_n$ , then

phases occurring between  $t_n$  and  $t_{n+x}$  showed a characteristic difference from those occurring between  $t_n$  and  $t_{n-x}$ , irrespective of the presence of any sentient being, whether as agent or observer. (Knowing and doing might themselves be regarded as sub-classes of just such a class of processes) All that would be required would be for these multiple non-symmetries to be capable of alignment in the same one dimension, and this would give us a time-order indicator.

An interesting example of a philosopher who looks in this fashion to the nature of things is Professor J. J. C. Smart, who wrote an article revealingly entitled "The Temporal Asymmetry of the World"<sup>1</sup> in which he maintained that the disparity between past and future depends on the existence of what he called *traces*. A trace is a changed state of affairs initiated by a specific occurrence which can be inferred very reliably from it; but while we can nearly always infer the *initiating* event of such a state, we can very seldom infer the *terminating* event. Footprints on the sand are very reliable evidence that someone has been walking on it; but the virgin sand is no evidence at all that someone *will* walk on it. This appeal to traces is very plausible, here we do seem to have an unambiguously past-pointing arrow, and there does seem to be no correlative future-pointing one, it is only in a very feeble and metaphorical way that coming events cast their shadows before. Yet we do speak of *signs*; and I am afraid that this appeal to traces is only another case of the commonplace distinction (2)—different degrees of reliability. There are signs which are more reliable than traces, even if this is exceptional. A counter-example to Smart's footprints would be a broken rail in front of an express train, where we can infer the future derailment with much greater certainty than we can infer the antecedents of the break.

Among those who look for differences in the nature of things, we must of course count the physicists who claim to find irreversible processes in the physical world which are time's arrow. We are told, for instance, that if we measure the temperatures of a pair of bodies isolated from external sources of energy and if we find that on occasion A their temperatures are nearly equal, while on another occasion B the warmer one is warmer and the cooler one is cooler, then the second occasion B is necessarily earlier than the first A, because according to the laws of thermodynamics such a system will always tend towards, and not away from, thermal equilibrium. On this it seems enough to say that, even if the physicists have identified an irreversible

<sup>1</sup> *Analysis*, vol 14, p 79.



process, it can only be regarded as correlated with, and not as defining, the time-direction, it does not seem self-contradictory to suppose that thermal energy should tend to become more and more unequally distributed in an isolated system, that the process might be reversed at any time, with no more drastic consequences than the breakdown of a physical law.

It does look as if we shall have to find the difference in ourselves after all, and not in the world. The notion of cause and effect is promising at first sight as a time-direction indicator, for it does seem as certain as anything can be that cause always precedes effect, and exceptionally preposterous to suggest that an effect might precede its cause. Yet it is not easy to see why, especially if we adopt an orthodox view about causation, namely that a cause is a sufficient condition for the occurrence of the event in question, and perhaps a necessary condition as well. For sufficient and necessary conditions are strictly neutral with respect to time. If A is a sufficient condition for B, then B is a necessary condition for A, for whenever we can say "If A occurs, then B occurs" we can also assert its logical equivalent "If B does not occur, then A does not occur". Similarly, if A is a necessary condition for B, then B is a sufficient condition for A. It is no use saying that A must be both a sufficient condition for B, *and earlier*, if it is to cause B, for this begs the question.

I am inclined to think that a good deal could be done with a more primitive notion than that of cause and effect, namely the notion of means and end. Cause and effect is observer's language, means and end is participator's language, and if we must in the end rely on agent-centred criteria, then the notion of means and end is the most promising, since it is much more deeply embedded in action than is the more theoretical notion of cause and effect, which was one of Ayer's candidates (let alone his other candidate, the still more theoretical notion of knowing). But I expect difficulties in the way of disentangling the notions of cause and effect, and of means and end, from each other, and also further objections of the cart-before-the-horse type—that what we are able to think of as means, or as ends, is already determined for us by the pastness or futurity of the events in question. Similar difficulties might attend yet another promising line of enquiry which would start from the difference between sense-experience, on the one hand, and such experiences as wishing, hoping and fearing, on the other. This enquiry would rely heavily on the non-indicative uses of language, especially on the optative and the imperative moods of verbs. It is still a worthwhile exercise, however, to try to reach the limit of what can be

done within the province of the indicative mood. and this is what I shall do. My route will take us over some logical territory, from which we shall emerge more or less where we started—at the concept of a particular speaker in a particular situation—but somewhat richer, I hope, for the journey.

## II

A block of stone, so Michelangelo is said to have said, already contains the statue that is going to be carved out of it. It also, of course, contains an infinite number of statues that are not going to be carved out of it. This is true in a whimsical but not outrageous sense. But the corresponding backward-pointing statement does seem to be outrageous. The existing statue was contained by the block out of which it was carved: but can we say, even whimsically, that it was also contained by an infinite number of blocks out of which it was not carved? Actual blocks, it seems, can contain possible statues, but can possible blocks contain actual statues? It does look as if the actualisation of potentiality is a one-way process; the possible can become the actual, but the actual cannot become the (merely) possible. Possibilities that are open can be closed, those that are closed can sometimes be reopened, but possibilities that have been closed in the special way of being realised cannot be reopened. We might try saying, then, that the future just is the region of the merely possible.

But surely it will not do to say simply that the future is the region of the possible. It will be objected that the past and present also contain possibilities, for we certainly do have a use for the expression 'It is possible that' followed by either past- or present-tensed verbs. As an example of a present-tense uncertainty: it is possible that a rocket is now on its way to the Moon. As an example of a historical uncertainty: it is possible that the Dark Lady of the Sonnets was Mary Fitton, or that she was Elizabeth Vernon, or somebody else. And it is no use our saying "Well it is certain that a rocket either is on its way to the Moon or it isn't only we don't know which", or that the Dark Lady was certainly either Mary Fitton or some other woman, only we don't know which—because exactly the same can be said of a future-located possibility: it is certain that there either will or won't be a sea-battle tomorrow, only we don't know which.

What can—indeed must—be said is this. Past- and present-located possibilities are actualised possibilities and, therefore,

irrevocably closed ; future-located ones are not. Of the two possibilities about the rocket, one of them is not merely possible, but actual ; and similarly with the two possible women who might have been the Dark Lady.

But does this say anything? Is the jargon of possibility and actuality any more than a verbal expedient ; does it really come to any more than that some possible happenings *have* happened, and some other, equally possible and otherwise indistinguishable happenings have *not yet* happened?

I think it does. But to show this I shall need to translate the somewhat archaic language of possibility and actuality into something less intractable. I shall suggest that the difference between speaking of a possibility as such, and speaking of a possibility as actualised, is just the difference between enunciating or formulating or entertaining a proposition, and asserting that proposition as true. In the case of the moon rocket, I can enunciate both the proposition that the rocket is on its way to the Moon, and the proposition that it is not, I can assert as true the (merely logical) proposition that either it is on its way or it isn't ; I cannot (though somebody else perhaps can) assert one of the propositions as true. In the case of the statue, the sculptor just about to start work can assert as true the proposition that this is an unchiselled block of marble ("actual block") ; he cannot assert as true any proposition about a statue made out of this block, but he can, or at least could in principle, enunciate a proposition containing a specification for a statue, such as a three-dimensional equation or a list of co-ordinates ("possible statue"). When at last he is in a position to assert as true a proposition stating that this statue is of such and such dimensions, possibility has become actuality. Now is this process irreversible, so that it could be used as a time-criterion? Before we can answer this question, we must distinguish at least four possible ways of interpreting the question.

(1) Is there a permanent logical asymmetry between propositions of a certain kind, that can only be enunciated but not asserted, and those of another kind which can be asserted as true? Obviously not. Propositions do not bear the marks of their own assertibility.

(2) Is there a necessary correlation (in something less than a logical sense) between the kind of processes exemplified by the sculptor's activities, and the process of coming to be in a position to assert a proposition which formerly could only be entertained? To see that the answer to this, too, is No, we have only to imagine

an eccentric sculptor whose speciality is the reconstruction of blocks of stone by encasing existing statues with concrete. In his case, the assertibility of the two propositions, about the statue and about the stone, is reversed.

(3) Is there a necessary correlation between statements about the present and past, on the one hand, with propositions that are assertible, and between statements about the future, on the other hand, with propositions that are not assertible? Again, clearly not, since we do claim that some propositions about events in the future relative to the time of assertion, *can* be asserted, and not merely entertained.

(4) Is there a necessary correlation between the two kinds of statement (past and present, and future) and two *types* of proposition, or two *ways* of asserting a proposition? The answer to this is Yes

To elaborate this, I shall start with something rather crude, which will get me as far as the thesis which Ryle adopts in the essay "It was to Be" (*Dilemmas*); but I shall not stop there, because Ryle's position, though correct in essentials, is highly vulnerable. He has selected examples which favour his analysis while ignoring others which seem to defy it, and which opponents such as Ayer<sup>1</sup> have been quick to seize on. In fact the critical cases are not intractable; but this remains to be shown.

### III

As a crude beginning, then, consider the two sentences.

(S1) The cat is drinking milk

(S2) A cat is drinking milk

Each of these sentences formulates a proposition which can, of course, be asserted as true. The important difference is that (S1), if it is asserted at all, is necessarily being used to describe an actual particular situation. This is the force of the demonstrative "the". (S2), if it is asserted, is not necessarily used to describe any particular situation. There must be, of course, if it is to be true, some actual situation such as (S1) would correctly describe; but this is just what (S2) says, and no more - just that there is, somewhere or other, an instance of cat-drinking-milk, that the descriptive expression "cat drinking milk" is satisfied.

<sup>1</sup> In an (unpublished) address to the Murhead Society, 1957.

Often and typically we use different modes of assertion for (S1) and (S2), and not just a change of article. We use the ordinary subject-predicate form as in (S1) *when we are able to pinpoint one of the actors* in a particular situation; we use the existential expression "There is . . ." when we wish to say that some type of situation is exemplified, as in (S2).

The proposal now is: All future-tensed statements are of type (S2) and none are of type (S1), whereas present- and past-tensed statements may be of either type. We can say indifferently, "William invaded England in 1066" or "There was an invasion of England by William in 1066": the difference is largely a matter of style and emphasis. But typically, past- and present-tensed statements are of type (S1). We usually are in a position to pinpoint the actual participators in historical and contemporary dramas. We are never in a position to pinpoint the actors in future dramas.

This last point will be challenged. Surely it just is not true that we are never in a position to pinpoint the actors in future dramas. This is the objection made by Ayer, who clearly thought it was a knock-out blow, against Ryle's thesis in "It Was to Be". Ryle discusses statements like "This accident could have been prevented" and points out that such a statement involves a sort of contradiction. For if the accident *had* been prevented, there would never have been *this accident* at all: neither I nor anyone could, logically, have prevented *it*, for *it* was an accident that was not prevented, and the fact that it was not prevented is already contained in the reference to *this accident*. What this points to is that words like *this* and *that*, personal pronouns and the whole apparatus of singular propositions—our pinpointing equipment—are out of place in talking about what can be prevented or promoted, and in general about the future. "Roughly," says Ryle, "statements in the future tense cannot convey singular, but only general propositions, where statements in the present and past tense can convey both. More strictly, a statement to the effect that something will exist or happen is, *in so far*, a general statement".<sup>1</sup>

Not that Ryle is the only sinner. Prior, in *Time and Modality*, admits that "it is very difficult to deny that a statement like 'Professor Carnap will fly to the Moon' is about Professor Carnap", yet he *does* deny it, holding (for reasons which I shall not discuss) that what does not yet exist cannot properly be named—and what cannot be named cannot be talked about.<sup>2</sup>

<sup>1</sup> *Dilemmas*, p. 27.

<sup>2</sup> A. N. Prior, *Time and Modality*, p. 33.

Half a century earlier C S Peirce made very similar remarks "We cannot assign proper names to each . . . of the possible or probable theatrical stars of the immediate future . . . the individual actors to which our discourse now relates become largely merged into general varieties", "there is an approach to want of identity in the individuals of the collections of persons who are to commit suicide in the year 1899".<sup>1</sup>

We can understand Ayer's impatience with all this Surely Prior's statement about Professor Carnap flying to the Moon really is about Professor Carnap and the Moon, both of which are here to be referred to, genuine individuals, yet also named as actors in a future drama? Surely it is flying in the face of common sense to insist that a singular future-tensed statement isn't really singular at all, isn't really about its ostensible subject?

These objections may look less overwhelming if we consider another case You may make plans for your child before he is born, or even conceived You may even give him a name, and put his name down for a public school Is there anything outrageous about saying "Johnny will go to Rugby" before Johnny is born, or even conceived? Not outrageous—just a somewhat reckless assumption that it will be a boy Less odd still, when he is conceived but unborn Less odd still, when he is born but still in nappies And so on But there is no line to be drawn At no stage do you reach the privileged position of being able to speak of a particular—until you have ceased to talk about the future

#### IV

The only way of breaking the deadlock as between the Ryle-Prior-Peirce thesis (hence RPP) and its opponents is to return to logic and say something more about types of propositions (singular, general) and modes of asserting them I shall choose ordinary simple subject-predicate propositions ("Socrates is wise", "Plato speaks"), not, however, for the sake of simplicity, but because these are the very cases that constitute a stumbling-block to the RPP Nearly all logical systems take singular propositions as elementary, and if no singular propositions can be about the future, as the RPP holds, then either logic has not succeeded in formalising statements about the future, or else such statements are somewhat more complex than they appear to be I shall choose the second alternative

<sup>1</sup> Quoted by Prior, *op cit* pp 113-114

In a proposition like "Socrates is wise" or "Plato speaks", we have two elements, a subject term and a predicate term. Neither of these can stand alone, if we are trying to say something that is true or false, they are strictly complementary; but neither of them logically requires any particular complement; we can say a lot of things about Socrates other than that he is wise, and we can say of other men than Socrates that they are wise. A predicate term, in general, can be attached to a number of different subject terms, and a subject term can have a number of different predicate terms attached to it. This is represented by what is usually called a propositional function but which I prefer to call a propositional schema

$$\phi x \dots \dots (1)$$

Conventionally the predicate is written first "x", with other letters from the end of the English alphabet, is a subject-term variable or individual variable, all that "variable" means is that where "x" stands in the schema you are to think of a blank space into which some subject term, such as "Socrates", must be entered if you want to make up a sentence. Similarly " $\phi$ ", with other letters from the end of the Greek alphabet, is a predicate variable, which means another blank space to be filled up with a predicative expression, like "is wise" or "speaks", if you want to complete your sentence.

Imagine both spaces in (1) filled by some appropriate expressions, such as "Socrates is wise". To show that a space has been filled by an appropriate expression, we use letters from the beginning of the English and Greek alphabets

$$\theta c \dots \dots (2)$$

This stands for an ordinary sentence which expresses a proposition which is true or false (the empty schema,  $\phi x$ , is not true or false)

Now suppose we have filled only one of the blanks

$$\theta x \dots \dots (3)$$

$$\phi c \dots \dots (4)$$

These formulae represent incomplete sentences, (3) says, for example, "— is wise", (4) says "Socrates —". Partly blank sentences, as opposed to totally blank ones, invite us to supply the missing part, (3) invites the question "Is there anybody who is wise, and if so who?" while (4) asks "Is there something that characterises Socrates, and if so what?" (4) will be ignored in what follows.

Now the question which the formula (3) invites falls into two parts, we answer the first part if we say, "Yes, there is someone

who is wise", but not the second part, to answer this we have to add, "namely, Socrates" or "namely. Socrates, Plato, Aristotle . . ." if there is more than one wise man. If the answer to the first part is "No, there is no one who is wise", then the second question is shown not to arise. The two answers to the first part, affirmative and negative, are symbolised thus

$$\exists x(\theta x) \dots \dots \dots (5)$$

$$\sim \exists x(\theta x) \dots \dots \dots (6)$$

(5) is to be read as "There is a value of  $x$  such that  $\theta x$  is true"; more informally, "It is possible to fill the blank space in the schema '— is wise' in such a way as to get a sentence which expresses a true statement"; more colloquially, "There is a wise man", or "Someone is wise" or "A man is wise" (compare "A cat is drinking milk").

The important feature to notice here is that (5) is a complete sentence, and expresses a proposition which is true or false, even though it contains variables, or blank spaces, and two blank spaces at that; whereas (3) which contained only one blank space was a mere schema and not a sentence at all. The logicians say that this is because (5) contains only bound variables, whereas (3) contains a free variable, but for our purposes it is enough to notice that (5), since it contains no singular terms, must be a general statement. When we proceed to answer the second part of the question, "namely, who?" by saying "namely, Socrates", we do introduce a singular term. But this singular term is *not* a subject term and the sentence remains essentially general:

$$\exists x(\theta x \quad x = c) \dots \dots (7)$$

This is to be read, say, "There is a wise man, namely, Socrates". In certain contexts, the sentence "Socrates is wise" (which is normally of type  $\theta c$  (2)) could be used to express what (7) expresses. namely, when it is the answer to the question "Who, if anyone, is wise?" This ambiguity—that "Socrates is wise" may be either of type (2) or of type (7)—will be important in the sequel.

## V

So far we have not introduced tenses. It is a natural assumption that in a singular statement the time-reference is carried by the predicate-term, since temporal relations are predicable like any other relations. If we have  $\theta c$ , say "Plato speaks", we might try to represent "Plato spoke", "Plato is speaking",



and "Plato will speak", by  $\theta^{-n}c$ ,  $\theta^0c$ ,  $\theta^{+n}c$ , where the superscripts indicate a temporally qualified predicate. But what is the temporal datum-line to which the superscripts relate? Obviously the utterance of the sentence in question, but this introduces a difficulty, since part of the meaning of a proposition is now being given by the context in which it is uttered. We must needs accept this complication, since statements about the future are statements about the future as determined by the time of stating. And we cannot avoid it by making the temporally qualified predicates relate to some standard event other than the utterance of the sentence, for example by using ordinary dates such as  $\theta^{399} B C$ , since this would merely be a statement about a time, not necessarily about a past or future time.

It is important to distinguish between the logical structure of propositions, and the conditions or contexts of asserting them which is an extra-logical consideration. The simple existential (5), since it does not refer to any particular event, does not refer to anything past, present or future, and can be asserted independently of the time-location of any event or events which happen to verify it. The simple singular (2) can be asserted of past and present events, but *cannot be asserted of future events* according to the RPP. This, if true, would constitute a time-order criterion, we should be in a position to say that those statements which we were unable to assert as singular propositions, but only as general propositions, were statements about the future; all other statements are about the present and past. The direction of time would be determined by the progressive accumulation of singular statements becoming available to match statements which hitherto had been only general. In the case of the sculptor, as the work proceeds (either in the orthodox or in the eccentric fashion) singular statements become available for assertion as the counterparts of general statements; a statement about the dimensions of a statue becomes available for assertion, and is (if things go according to plan) the counterpart of a general statement such as a list of co-ordinates.

The RPP is disputed. But perhaps its opponents have failed to observe the fact on which all the above hinges, that from the occurrence of a singular term in a sentence it does not follow that that sentence expresses a singular proposition. We have seen how the apparently singular proposition "Socrates is wise" can be a general proposition of type (7), if it occurs as the answer to the question "Who, if anyone, is wise?" Similarly, we have to see that an apparently singular proposition about the future, such as "Professor Carnap will fly to the Moon", may well be a

general proposition in disguise. It could be the answer to a question "Will anyone fly to the Moon, and if so who?" More formally, "There is a value of  $x$  such that ' $x$  flies to the Moon' are both true", and ' $x$  is Professor Carnap'.

What are the reasons for insisting on this move? Simply the difficulty which Ryle notices - the difficulty of seeing how we can talk about "what we have not got" (to use his own calculatedly crude expression) more precisely, the difficulty of seeing how the conditions for assertibility can be satisfied. For a subject-predicate sentence to have a meaning, to express a proposition which can be true or false, it is necessary that it should contain a subject term. For such a proposition to be *asserted*, a further condition must be satisfied. the subject term, which purports to name some particular thing, must actually name some particular thing - it must be given a reference. The difficulty of giving a reference to singular terms occurring in future-tensed statements is not obvious in the case of the "Professor Carnap" example, just because we have indeed got Professor Carnap, and opponents of the RPP trade on just such examples as this. But it remains to be pointed out that, although we have "got" the present Professor Carnap, we have not "got" the future Professor Carnap, which is what the sentence is supposed to be about. The difficulty of giving a reference is certainly still there, however impatient we may be at the pedantic discrimination between two Professor Carnaps—what's the difference? But the difficulty is obvious, and the discrimination surely not pedantic, in the strictly analogous case of our imaginary unborn son who is going to go to Rugby. What my statement purports to refer to either doesn't exist at all, or exists in some pre- or post-natal stage of growth; it certainly is not the future schoolboy, whom I have not "got". The sentence cannot refer at all and must, therefore, if asserted at all, be asserted as a general statement, as the RPP holds. When you say "My son will be at Rugby" what you are saying is that there will be some individual who satisfies both the descriptive expressions "being my son" and "being at Rugby" (and, if you like, "being named Johnny"). But just so, when you say "Professor Carnap will fly to the Moon", what you are saying is that there will be some individual who satisfies both the descriptive expressions "flying to the moon" and "being identical with Professor Carnap"—where "being identical with" conceals no logical tricks but merely refers to ordinary personal continuity. There is no difference in principle between the case of our future Moon-traveller being closely similar to, though more aged than, the present distinguished professor, and the case of the future

Rugby schoolboy being more aged than, though *not* closely similar to, a present-day infant. In neither case have we "got", as Ryle would say, the actual subject of our discourse.

Certainly Ryle's talk of what we have and have not "got" is provocatively slipshod and demands refinement. We might well want to say that there is quite a lot of the present that we have not "got" either, and even more of the past, perhaps the whole of it. I can only roughly hint at possible ways of meeting that challenge. All statements in the past or present tense can in principle be tied down to sense-experience; they contain referring expressions as well as descriptive ones, or where they do not—where type (S2) statements occur—they occur in a framework of discourse which does typically contain referring expressions, type (S1). Such referring expressions always carry with them a context of sensation. I shall not go into the question whether sensations must be distinguished from memory—whether all experience is memory, or whether there is a specious present as distinct from memory, whatever the answer to this, it seems undeniable that the phenomenal present shades off into the phenomenal past in contrast with the sharp break between present and future. There is no phenomenal future.

But where sense-experience leaves off, wishes, hopes and fears remain. I have already hinted that this is the best place to look for an unambiguous pointer. It is no use crying over spilt milk; but unspilt milk is a proper thing to cry for or against. Prayers, incantations and imprecations, however irrational they may be, are at least not irrational in the special sense in which lamentation is (and perhaps remorse). Or to put it in a more linguistic vein, the indicative mood does indeed span all three temporal regions, but its proper home is in the present and the past, and its resources are stretched to the limit when it enters the region proper to the imperative and optative moods.

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## II.—INTENTIONALITY AND EXISTENCE

BY WILLIAM W. ROZEBOOM

JUDGED by the number of recent articles on the topic of "existence", this hoary problem in analysis is as controversial as ever. It is my suspicion that much of the difficulty which has arisen here stems from two roots—an undercritical and unwarranted use of certain formal transformations of expressions, and a confounding of two very different semantical concepts. I shall try to establish the following points (a) Singular existence-statements of the form '*A* exists' (not to be confused, when '*A*' is a predicate, with the general existence form '*A*'s exist') or '*A* does not exist' are *intentional* in that sense (Brentano) where '*John* believes that all swans are white' and '*John* is thinking about centaurs' are intentional while '*John* is tall' is not (b) There appears to be a non-intentional, or "objective", analogue of '*A* exists', but not of '*A* does not exist'. (c) There is no quicker way to overpopulate one's ontology than by confusing "meaning" with "reference", a point which has already been forcibly argued on several occasions by Quine.

The present account of existence-statements will concern only those which arise in regard to expressions which syntactically are substitution instances of variables. Whether there are meaningful singular existence-statements which are not of this kind is problematic. By the phrase, "*A*' is a descriptive term of language *L*," let us mean that there is a variable, '*Φ*', in *L* such that if '*F(A)*' is a sentence in *L*, the syntactical rules of *L* authorize passage from '*F(A)*' to ' $(\exists \Phi)F(\Phi)$ ' or, what amounts to the same, from ' $(\Phi)F(\Phi)$ ' to '*F(A)*'. (Note that "*A*' is a descriptive term of *L*," so defined, is a purely syntactical concept which says nothing about the *semantical* properties of '*A*') Then our present concern is with statements of form '*A* exists' when '*A*' is a descriptive term. Since the analysis applies as well to the existence of abstract entities as to that of particulars, it is convenient to assume that the language under consideration contains variables of more than one logical type. However, this is *not* to assume that *all* well-formed expressions are syntactically substitution instances of variables. In particular, we leave open the question whether or not compound expressions such as complex predicates and entire sentences are descriptive terms as here defined.

The above declaration of intent to structure problems of existence in terms of the use of bound variables may lead the

reader to anticipate similarities between the present views and those already aired by Quine. This suspicion will be substantially confirmed; however, the Quinian account has certain lacunae which the present discussion will try to fill.

A terminological explanation. It will be noted that with a few special exceptions, the term 'intentional' (in its non-theological sense pertaining to meaning and aboutness) is used throughout this paper rather than the more familiar 'intensional'. While the latter term is frequently imbued with the sense of the former—some philosophers, for example, explicitly equate "intensions" with "meanings"—the various definitions that 'intensional' has received in related but significantly different contexts have made this a dangerously ambiguous word which is likely to confuse, rather than clarify, at critical junctures in an ontological or semantical analysis. An example of this confusion will be pointed out in Section V, below

# I

I shall launch my argument with the contention, to be justified only much later, that subject to one possible reservation, a satisfactory objective version of "*A* exists" is

$$(1) \quad (\exists \Phi)(\Phi = A),$$

where '*A*' is a syntactically permissible substitution instance of the variable ' $\Phi$ '. Formula (1) is applicable (with appropriate adjustments of ' $\Phi$ ') to descriptive terms of all logical types, and in the case where '*A*' is a predicate, implies *not* that *A* is exemplified (as would be expressed by the form ' $(\exists x)Ax$ ') but that *A* has existence in its own right—*e.g.* not that there are red objects, but that Redness exists. I have chosen (1) as an objective assertion of existence because it may be read, "There is something which is identical with *A*", and has thus a good, solid existential feel. Actually, as will be seen, a number of other forms would do as well.

Now, at first blush, (1) might seem immediately open to the fatal criticism that it attributes existence not only to real entities, but to imaginary ones as well. For cannot we deduce

$$(2) \quad \text{Pegasus} = \text{Pegasus}$$

from the logical truth

$$(3) \quad (x) (x = x)$$

and from thence infer

$$(4) \quad (\exists x) (x = \text{Pegasus}),$$

thus showing that if (1) implies that  $A$  exists, then Pegasus exists? But this attack is invalid. What it actually reveals, I shall argue, is that a statement ' $F(A)$ ' is logically entailed by a universal generalization ' $(\Phi)F(\Phi)$ ' *only conditional on the existence of  $A$* . That is, ' $(\Phi)F(\Phi)$ ' is not in itself a sufficient condition for the conclusion ' $F(A)$ ', a sentence which implies that  $A$  exists is also needed

It is easy for the modern philosopher, well versed in the intricacies of formal logic and appreciative of its powers as a conceptual tool, to fall into the trap of assuming that a sentence which is formally *valid* must also be *true*. But the definition of "formal validity", roughly speaking, is that a sentence is valid if and only if it comes out true under any assignment of designata to its non-logical terms. This is a purely *syntactical* property of a sentence, and is wholly independent of whether or not all its non-logical terms *have* designata, or even of whether or not the sentence is meaningful. That is, formal validity is a truth-disposition—if a sentence is formally valid, a sufficient condition for it to be true is that all its non-logical terms designate. Thus

- (5)  $Bik = Bik,$   
 (2)  $Pegasus = Pegasus,$   
 and  
 (6)  $Chicago = Chicago$

are all formally valid and *would* be true if ' $Bik$ ' (a nonsense-syllable), ' $Pegasus$ ', and ' $Chicago$ ' all were to designate something. But while (6) is hence unquestionably true, (5) is meaningless and (2), I shall argue later, is false.

Precisely the same situation obtains for formal deducibility. Roughly speaking, a sentence  $S_2$  is validly deducible from a sentence  $S_1$  if and only if  $S_2$  is never false when  $S_1$  is true under an assignment of designata to their descriptive constants. Hence if  $S_1 \vdash S_2$ , the truth of  $S_1$  guarantees the truth of  $S_2$  only if the truth of  $S_1$  also guarantees that all non-logical terms of  $S_2$  meet certain minimal *semantical* standards. Hence we cannot *logically* (contrasted to *formally*) deduce (2) from (3) alone, but only from (3) and some additional premise which implies, though it need not assert, that the sign design ' $Pegasus$ ' possesses those minimal semantic properties, whatever these may be, necessary for a well-formed formula containing ' $Pegasus$ ' to be capable of truth.

Before plunging into more controversial matters, let me review the argument so far. It has been pointed out that a *formal* deduction is not a *logical* deduction, which vouches for the truth

of the conclusion given the truth of the antecedents, unless the truth of the antecedents also insures that all non-logical terms of the conclusion satisfy a certain semantic requirement. (From this, we see that the fact that (4) is formally deducible from (3) does not prove that (4) is *true*, and hence does not testify against the acceptability of (1) as an objective analysis of "*A* exists".) Now to this there can surely be no objection, for obviously we must exclude meaningless sentences such as (5) from the logical truths. But it is one thing to recognize that a descriptive term '*A*' must possess a certain minimal semantic property,  $\Sigma$ , where

- (7)  $\Sigma(s) \equiv s$  is syntactically a descriptive term such that for any matrix ' $F(\ )$ ', if ' $(\Phi)F(\Phi)$ ' is a true sentence in which ' $\Phi$ ' is a variable of which  $s$  is syntactically a substitution instance, the sentence formed by replacing ' $\Phi$ ' by  $s$  throughout ' $F(\Phi)$ ' is true,

in order that '*A*' be able to occur in a true sentence, and entirely another to say what the property  $\Sigma$  is. I believe that an unexpressed premise of practically everyone who has discussed the problem of existence, with the notable exception of Quine, has been that any formally tautological sentence, if meaningful, must be true, and hence that ' $\Sigma('A')$ ' must have the force of

- (8) '*A*' is a meaningful descriptive term.

But if this assumption is not justifiable—if (8) is *not* a sufficient condition for  $\Sigma('A')$ —then it is plausible that  $\Sigma('A')$  obtains in just those instances where it is correct to say that *A* exists. That the latter is indeed the case is what I shall now attempt to show

My first contention to this end is that if ' $F(A)$ ' is a sentence in which '*A*' occurs descriptively, then a necessary condition for ' $F(A)$ ' to be *true* is for '*A*' to have a referent. That is, I suggest that

- (9)  $(s) [\Sigma(s) \equiv (\exists \Phi) (s \text{ designates } \Phi)]$ .

There are at least two lines of argument which may be adduced to support (9), one negative and the other positive. The first is that what formal logic tells us about a formally valid sentence *S* is only that *S* must be true *if* all its non-logical terms have designata. Formal considerations give no reason for presupposing that *S* need be true when this semantical condition is unfulfilled, in fact, classical formal analyses have deliberately avoided discussion of this contingency by, *e.g.* arranging for a definite description always to have a unique referent. The second argument is one which I shall exhibit but not attempt to develop

here in detail. Recent analyses of the pragmatic force of concepts have given increasing weight to the possibility that the very use of concepts entails certain empirical commitments. Thus certain contemporary philosophers, notably W. Sellars, have insisted that adoption of a concept is correct or incorrect according to the way in which the world is put together, and I have elsewhere<sup>1</sup> tried to show that a necessary condition for the truth of any statement using theoretical terms introduced by a scientific theory is the existence of entities which do, in fact, exemplify the observational properties ascribed by the theory to the alleged referents of its theoretical terms. When a term, *s* has such built in existential commitments—and it is not impossible that this is true of *all* meaningful descriptive terms—assertion of *any* statement, not excluding tautologies, of which *s* is a constituent embodies commitment to the additional premises supporting *s*, and must hence be in error if these premises are not realized. And since the effect of such failure is to deprive *s* of a referent, we thus have a semantical situation in which a necessary condition for the truth of a statement containing *s* is that *s* have a designatum.

The direction of this argument may be illustrated through a somewhat controversial example. Suppose that 'Pegasus' has been defined as 'the winged horse [that, etc.]'—*i. e.*

$$(10) \quad \text{Pegasus} =_{\text{def}} (x)(Wx \cdot Hx)$$

Then, if we accept Russell's analysis of definite descriptions, namely,

$$(11) \quad \Psi[(x)\Phi x] =_{\text{def}} (\exists x)(\Psi x \cdot (y)[\Phi y \equiv y = x]),$$

we see from (10) and (11) that

$$(12) \quad (\text{Pegasus} = \text{Pegasus}) \equiv (\exists x)(Wx \cdot Hx),$$

and hence that (2) is false. Now as it stands, this is not very exciting philosophically, for (11) claims that statements incorporating definite descriptions are not in logically proper form and that in particular, definite descriptions are abbreviatory ellipses rather than proper descriptive terms and hence not syntactically substitution instances of variables. Thus under (10) and (11)

$$(2) \quad \text{Pegasus} = \text{Pegasus}$$

is *not* a substitution instance of

$$(3) \quad (x)(x = x).$$

However, it can also be maintained<sup>2</sup> that while ' $\Psi$  (Pegasus)'

<sup>1</sup> W. W. Rozeboom, "The factual content of theoretical concepts", in H. Feigl and G. Maxwell, eds., *Minnesota Studies in the Philosophy of Science*, vol. III (University of Minnesota Press, 1960) <sup>2</sup> *Ibid.*



and ' $(\exists x)(\Psi x \cdot (y)[Wy \cdot Hy \equiv y = x])$ ' have the same *truth conditions*, the latter is not an *analysis* of the former, but that the logical form of ' $\Psi$  (Pegasus)' is ' $\Psi x$ ' and that 'Pegasus' is a meaningful descriptive term which *designates* an entity  $x$  if and only if  $(y)(Wy \cdot Hy \equiv y = x)$ . If this be granted, it follows that ' $\text{Pegasus} = \text{Pegasus}$ ' is formally valid, but commits its believer to a falsehood, namely ' $(\exists x)(Wx \cdot Hx)$ ', and hence cannot itself be *true*.

I thus conclude—not merely in virtue of unsatisfied definite descriptions, but on the basis of more general considerations which this case merely illustrates—that a descriptive term  $s$ , even though meaningful, may be a constituent of a *true* statement only if  $s$  has a referent—that (9) is the case. However, before contending further that  $A$  exists when and only when ' $A$ ' has a designatum, it would first seem desirable to say a little more about the distinction between Meaning and Reference, for confusion between these two notions, even by philosophers who have been acutely aware that a distinction must be made, has been responsible for a great deal of philosophical perplexity.

## II

There appears to be substantial agreement among serious students of the philosophy of language that at least two distinct semantical concepts are necessary for an adequate analysis of the aboutness of symbols. We need to speak of the "sense", "meaning", "concept", "connotation", or "intension" associated with a descriptive term on the one hand, and its "nominatum", "denotatum", "extension", "designatum", or "referent" on the other. It is by no means the case that these terms are all unambiguous, or are fully inter-synonymous within each cluster, in fact, I will argue later that the grouping as I have given it, though in accord with contemporary usage, contains a serious ontological error. Nonetheless, the necessity for drawing some such distinction seems inescapable.

The distinction between the meaning and the referent of an expression was first made explicit by Frege,<sup>1</sup> who pointed out that while 'The morning star' and 'The evening star' are two expressions with the same *referent*—namely, Venus—they differ from each other (and also from 'Venus') in their *meanings*, as demonstrable by their failure to be interchangeable in certain "indirect"

<sup>1</sup> G. Frege, "On sense and reference", in P. Geach and M. Black, eds., *Translations from the Philosophical Writings of Gottlob Frege* (Oxford Blackwell, 1952).

contexts such as 'John doubts that—is identical with the evening star'. More generally, if '*A*' is a singular descriptive term of the language we are using, then the referent of '*A*' is given by the statement

(13) '*A*' designates *A*,

which is true so long as '*A*' has a referent. Thus,

(14) 'Venus' designates Venus,

(15) 'The morning star' designates the morning star,  
and

(16) 'Triangularity' designates Triangularity.

The need to stipulate that '*A*' has a referent is to exclude cases such as

(17) 'Pegasus' designates Pegasus,

the truth of which would necessitate the existence of a winged horse. If certain *non*-singular expressions, such as adjectives, are also regarded as descriptive terms, the grammar of 'designates' dictates that assertions about their referents be obtained from the associated singular forms; for example,

(18) 'Triangular' designates Triangularity,

the truth of which, of course, still presupposes the existence of the abstract entity, Triangularity. (This transition from the non-singular to the singular form of a predicate is a tricky business which warrants substantially more discussion than is practical here. One way to support (18) is to contend that if 'triangular' is syntactically descriptive, the true logical form of '*a* is triangular' is '*a* exemplifies Triangularity' or some variation thereof, and that to quantify over 'triangular' is really to quantify over 'Triangularity'.) Further, we note that for any *x*, *y*, and *z*, if *x* designates *y* and *y* = *z*, then *x* designates *z*. Thus given that the evening star is identical with the morning star, we have from (15) that

(19) 'The morning star' designates the evening star.

On the other hand, while meaning-talk bears a superficial resemblance to reference-talk, a little reflection shows there to be something a bit queer about the form. '*x* means *y*'. We generate a true statement about the meaning of *any* meaningful expression '*A*' in our language, whether '*A*' is a singular descriptive term or not and without additional assumptions about empirical reality, by asserting that

(20) '*A*' means *A*,

where the second substitution in '*x* means *y*' is an *exact* trans-  
 literation of the first except for (a) the absence of quotes, and  
 (b) the presence of a contextual signal (the boldface in (20), the  
 italics subsequently) that the expression is being used in a special  
 way.<sup>1</sup> Thus,

(21) 'Venus' means *Venus*,

but also

(22) 'Pegasus' means *Pegasus*,

(23) 'Triangular' means *triangular*,

and

(24) 'And' means *and*

It is clear that despite the identity of the morning star with the evening star, 'The morning star' does not mean *the evening star*. Nor does the truth of (22) or (24) presuppose, respectively, the existence of a winged horse or an abstract entity, And-hood, designated by 'and'. The conclusion is inescapable that the italicized expressions in (21)-(24) are not playing their normal roles. In particular, the referent, if any, of 'Venus' in (21) is not the referent of 'Venus' in (14), nor is the referent of 'Pegasus' in (22) the winged horse that would be designated by 'Pegasus' in (17) if there were such a creature. Whatever ultimate interpretation we wish to make of the sentence-form '*x* means *y*', there can be little doubt that the meanings of descriptive terms are *not* their referents

What can we say about the relation between meanings and referents? It seems to me to be obvious that a descriptive term has a referent, if any, *because* of its meaning, while the relation between symbol and meaning is not a relation of aboutness. A symbol does not *refer to* its meaning, it *has* a meaning, *in virtue of which* it may refer to something else (That it is the meaning of a term which determines its referent, if any, may be seen by reflecting that two terms with the same meaning must necessarily have the same referent) This point becomes especially clear if we replace 'meaning' with 'concept', for we customarily say that a term *expresses* a concept of something. The semantical picture sketched by such a formulation portrays a symbol as *producing*, *bringing forth* or *arousing* (i.e. "pressing out") its meaning, while it is the latter which does the actual referring. Since the primary relation of aboutness would thus seem to make its appearance between the meaning and the referent of a term,

<sup>1</sup> The writings of Wilfred Sellars, too numerous to cite individually, are very important for clarifying the grammar of '— means —'.

I shall henceforth allow myself to speak without apology of entities being referred to by meanings. We may then say that when a linguistic expression  $E$  refers to an entity  $e$ , it is because there exists a meaning  $m$  such that  $E$  has ( $\epsilon$   $e$ . produces, arouses, expresses)  $m$ , and  $m$  refers to  $e$ .

This description of the relations among symbols, meanings, and referents is, in fact, very suggestive as to the nature of meanings. For it is evident on other grounds that a stimulus-pattern of shapes or sounds is a full-blooded cognitively meaningful symbol for a person only if that person has acquired certain language habits with respect to that stimulus-pattern. We may take it to be an empirical fact that a language user's transactions with linguistic entities produce (arouse, activate) in him certain behavioural (or mental) states which are, in some sense still very much in need of clarification, "appropriate" or "relevant" to the referents of the expressions in question, or to what *would* be their referents were the latter to exist. It is therefore most tempting to identify the meaning,  $m$ , of a symbol  $s$  as some aspect of an internal state characterized by the linguistic role of  $s$ , such that  $s$  designates an entity  $e$  if and only if  $e$  stands in a certain pragmatic relation to  $m$ . In fact, unless there are good reasons to the contrary, such an identification would seem to be dictated on grounds of parsimony alone, for consideration of the facts of language behaviour lead inexorably to the (scientific, not philosophical) conclusion that there *are* internal states generated by the use of language, and it is an unnecessary multiplication of entities to introduce the meaning of a term as something which differs *both* from its referent and from part of the internal state produced by its use.

For present purposes, however, it is unnecessary to argue for any particular interpretation of meanings, so long as it is agreed that the meaning of a term *is* different from its referent (though, of course, the meaning of one expression may be the referent of another), or, phrased somewhat differently, that the relation between a term and its meaning is not a relation of reference. For then it becomes wholly gratuitous to assume, as seems to be implicit in the views of a great many philosophers, that a meaningful descriptive expression must necessarily designate something. Not only can no *reason* be given for such an assumption, it entails such bizarre ontological commitments as to the existence of Square-circleness, and "possible" but not real facts designated by false statements (see Section V below). I strongly suspect that it is primarily the desire to disavow this "torrent of universals" that drives the nominalist to the (in my opinion) logically

inconsistent position that no abstract entities exist at all. But neither of these unpalatable extremes has any intuitive plausibility. It is easy to conceive that from a set of meaningful descriptive terms, all of which, say, have designata, one might construct a complex descriptive expression which has meaning because its constituents have meaning, but which need not itself designate anything. This is a much more natural interpretation of, *e.g.* a definite description than to assume that descriptions do not *really* refer, and that sentences containing them are syntactical anomalies. Similarly for descriptive expressions of higher logical type it does not follow that because we can construct the meaningful expression, "the class of red circles", or "the property of being a red circle", there must necessarily *be* such a class or property.

### III

In light of these remarks, let us examine the force of saying

(25)  $A$  exists,

or

(26)  $A$  does not exist

such statements have puzzled philosophers because while (25) and (26) seem to have empirical content, if we try to interpret them as we would 'John is tall', namely, as assertion that the entity  $A$  exemplifies a certain property, then either (25) is tautologous and (26) self-contradictory, or we have to assume that entities come in two styles—those which "exist" and those which do not. However, we have just argued that given a meaningful descriptive expression ' $A$ ', it is empirically significant to ask whether or not ' $A$ ' designates anything. Thus it is very tempting to suppose that (25) is an elliptical way to assert

(27)  $(\exists \Phi)(A \text{ ' designates } \Phi)$ ,

and that (26) is to be analysed as the negation of (27).

But this will not quite do. For as Church's translation test shows, (27) is a statement about the symbol ' $A$ ', whereas if (25) is a descriptive statement at all, it is not *about* ' $A$ ', but *uses* ' $A$ ' to talk about something else. Yet if we hold that 'John' in 'John exists' refers to the same entity that it refers to in 'John is tall', we are hard pressed to know what to make of 'Pegasus does not exist', since if there *were* exactly one winged horse, 'Pegasus' would then refer to it and hence in fact does *not* have a referent in this sense.

But there is no reason to suppose that a given symbol always has the same referent, if any, no matter what its context of usage. Actually, the evidence strongly suggests that there are two basic kinds of declarative linguistic contexts, the "intentional" and the "objective", such that the *referent* of an expression in an intentional context is its *meaning* in an objective context, whereas the latter contexts are simply those in which the expressions play their normal roles—*i. e.* where their meanings are what we would understand by them in the absence of cues for special usage. Thus 'John believes that all swans are white' and 'John is thinking about centaurs' are to be understood, given the aforementioned interpretation of meanings, as statements about John's behavioural (or mental) state. Similarly, the peculiarity of '*x* means *y*' is no longer mysterious, '*y*' here simply marks an intentional context and the italicized terms in (22) and (24) do not attempt to refer to a mythical Pegasus or an even stranger And-hood, but instead designate, respectively, the meanings of 'Pegasus' and 'and'. There is, of course, nothing new about this theory of contexts. Frege<sup>1</sup> said as much, and the only reason for adopting the present terminology, rather than his, is that "intentional-objective" more clearly characterizes the nature of the distinction than does "indirect-direct". But what I now want to suggest is that statements of forms (25) and (26) are also intentional contexts of '*A*'. What (25) then asserts is that a certain meaning, specifically, the one possessed by the term '*A*' when used in objective English contexts stands in a referential relation to some other entity—hence implying, though not asserting, that any symbol, '*A*' in particular, which has this meaning also has a designatum. Similarly (26) denies this claim. Thus (25) and (26) are empirically significant though they *mention* no symbols nor presuppose no realm of "possible but not actual" beings.

## IV

In my opening remarks, I contracted to find an objective analogue of '*A* exists' and moreover, alleged that (1) might be such a statement. We are now in position to see why this should be so. By an "objective analogue" of (25), I mean a statement with roughly the same force as (25) but which uses the expression '*A*' objectively. Now, we have just seen that '*A* exists' and '*A* designates' seem to be equivalent in so far as it is possible

<sup>1</sup> *Op cit.*

for a statement which uses an expression to be equivalent to one which mentions it. Hence any sentence ' $F(A)$ ', which uses ' $A$ ' objectively, is an objective analogue of (25) if a necessary and sufficient condition for ' $F(A)$ ' to be true is that ' $A$ ' have a referent. Now, it was argued earlier—specifically, it follows from (7) and (9)—that if ' $F(\Phi)$ ' is any formally valid formula in which ' $\Phi$ ' is the only free variable, and ' $A$ ' is syntactically a substitution instance of ' $\Phi$ ', then ' $F(A)$ ' is true if and only if ' $A$ ' has a designatum. Hence, any formal tautology in which ' $A$ ' occurs objectively as its only descriptive term is an objective analogue of ' $A$  exists'. If Identity is an objective context of its arguments, then (1) is such a sentence. Presumably, there are many others, such as ' $A = A$ '.

Similar considerations show there can be no objective analogue, ' $G(A)$ ', of ' $A$  does not exist'. For ' $G(A)$ ' must then be a sentence in which ' $A$ ' occurs objectively and which is true when and only when ' $A$ ' has no designatum. But this is impossible, since it is a *condition* for the truth of ' $G(A)$ ' that ' $A$ ' have a referent. In particular, if identity-assertions are objective in their subject-terms, the negation of (1), say in the case of Pegasus,

(28)  $(x)(x \neq \text{Pegasus}),$

cannot be analogous to

(29) Pegasus does not exist

because (29) is presumably true and (28) cannot be—in addition to the arguments by which (9) was supported, we would have, if (28) were true, the curious instance of a sentence which is true, but formally invalid.

## V

While this concludes the body of my argument, there are still a couple of loose ends which need to be tied off. The first has to do with the truth-status of meaningful statements which contain descriptive terms which have no designata. Traditionally, meaningful declarative statements are classified as either true or false. But statements such as 'Pegasus is winged', containing designatum-less descriptive terms, cannot be true. May we then consider them to be false, or must we introduce a new truth-category to deal with this case? This is probably for the most part a matter of terminological convenience, however, rather than create a special semantic limbo for such lost souls, I would pass a sterner judgment and damn them as simply false. For a

sentence containing meaningful descriptive terms which fail to designate usually if not always implies a belief which is orthodoxly false. Thus a person who believes 'Pegasus is winged' is thereby committed to belief in the existence of a winged horse. Moreover, the designata of true statements are undoubtedly facts. (The view that sentences refer to truth-values will not stand up under close analysis of discourse about events, causal relations, happenings, etc. This error stems from the mistaken identification of facts with true propositions. But surely it is more correct to say that propositions are intentional entities, specifically, that they are the *meanings* of declarative sentences, whereas facts are objective, specifically, that they are what true sentences are *about*.) If so, then what is a false sentence if not simply a meaningful sentence which fails to designate a fact? But containing a descriptive term which has no referent is a sufficient condition for a sentence not to designate a fact. Hence I submit that 'Pegasus is winged' and others of its ilk are unqualifiedly false.

The other loose end hangs from the existence of abstract entities, for while I have alleged that the present views provide the tools with which to give the Augean stables of platonistic ontology a good scrubbing without sacrifice of essential livestock, I have so far done little to justify this claim. Since somewhat different things need to be said about different categories of abstract entities, the present remarks will be limited to the problem of *properties* or *attributes*, commitment to the existence of which is presumably carried by the use, as descriptive terms, of singular expressions formed from predicates—e.g. 'Triangularity', 'Sweetness', 'Hardness', 'Red-squareness'. I would like to suggest that the existence of Hardness, Red-squareness, etc., may be an *empirical* question in precisely the same way that the existence of Chicago or Pegasus is empirical.

To begin with, we recall that discourse about "properties" differs from that about "classes" in that two properties,  $\Phi$  and  $\Psi$ , may be distinct even though co-extensive—i.e. that  $(\Phi x \equiv \Psi x)$  does not entail that  $\Phi = \Psi$ . Further, we assume that if non-singular predicates are to be treated as descriptive terms, they are to be considered ontologically equivalent to their singular form. Thus ' $x$  is red' is to be taken as equivalent to ' $x$  exemplifies Redness' or ' $x$  exemplifies Being Red'. (Note that this assumption is justified by ordinary usage in that if one were asked to list the properties of a hard, red object, the grammatically correct answer would be 'Hardness and Redness', rather than 'Hard and red'.) The property-commitments of a compound predicate in informal discourse are likely to be



ambiguous. Thus ' $x$  is a red square' can be interpreted either ' $x$  exemplifies Redness and  $x$  exemplifies Squariness' or ' $x$  exemplifies Red-squariness'. Formally, the scope of the abstraction operator ( $\lambda$ ) easily distinguishes these alternatives, the ontological differences of which will be pointed out below.

We now ask what the semantical relation is that a property bears to its corresponding predicate in ordinary contexts. At first glance, the answer would appear to be obvious—the earlier discussion of Reference seems to show without further ado that 'Redness' *designates* the property Redness if the latter exists. However, there is another interpretation which holds that properties are the *meanings* of predicates, while the referent of the predicate is then taken to be the class of entities which exemplify the corresponding property. This view, our legacy from Conceptualism, is reflected and insidiously propagated by classical terminology, which applies the term 'intensions', with its strong mentalistic connotations, to properties. Despite its widespread acceptance, the conceptualistic interpretation of properties seems to me to be wholly untenable.

(2) We have already seen that the sentence-form ' $\_$  — designates —' apparently calls for the mention of a descriptive expression in the first blank, and the use of its singular form in the second. If properties are no exception to this rule, then 'Redness' designates Redness. Hence if what a singularized predicate designates is a *class*, then Redness must be the class of red entities. But this is just what property-talk does *not* allow us to say—if there is any point to the property-class distinction at all, it is that in some important sense, the property Redness is distinct from the class of red entities. However while this urges that singularized predicates do not designate classes, it does not suffice to prove that properties are not meanings, for it could be suggested that while, *e.g.* Redness is indeed a meaning, it is not the meaning of the singular term 'Redness', but of some other expression. This possibility will be discussed further in (iv).

(ii) If properties are the meanings of predicates, then either the properties of objects are internal states of a language user, or meanings are shadowy extralinguistic beings lying around in wait to be grasped by the mind's hand.

(iii) We saw earlier that it does not seem correct to say that an expression is *about* its meaning. Rather, an expression *expresses* —*i.e.* calls forth, produces, arouses—its meaning, while it is the latter which does the business of referring. If so, the thesis that properties are meanings entails that predicates are not *about* properties, but *arouse* properties which then refer to the cor-

responding classes. Now it must be admitted that some philosophers apparently find this to be a congenial way of speaking. Church,<sup>1</sup> for example, has defined a property to be the concept of a class. Nonetheless, I submit that there is something a little strange in saying that a property is *about* a class. Such a view, moreover, would entail that the relation of an entity to its properties is a semantical relation, for then ' $x$  exemplifies  $\Phi$ ' could be analyzed as ' $(\exists c)(\Phi$  refers to  $c$  and  $x$  is a member of  $c$ )'.

(v) It is clear that at least *some* occurrences of singularized predicates refer to properties (if these exist) and not to classes. Consider, for example,

(30) Redness is a colour.

If 'Redness' in (30) designated the class of red things, then (30) would assert that the class of red things is a colour. Worse, it would follow that if the class of red things were identical with the class of square things, Squareness would be a colour. Moreover, that the property Redness (if it exists) is the referent, not the meaning, of the first word in (30), may clearly be seen by reflecting that 'Redness' may be replaced in (30) without change of factual reference by the expression 'The colour of ripe tomatoes', which differs in meaning but not in referent from 'Redness'. (To show that 'Redness' and 'The colour of ripe tomatoes' do, in fact, have the same referent in this context would call for greater discussion than is necessary here.) Once it is established that 'Redness' in (30) refers to Redness, it is then not difficult to show that except for obviously modal or mentalistic contexts, most if not all occurrences of singularized predicates apparently *refer to*, rather than *mean*, the corresponding property. For example, the fact that we would regard the conjunction of (30) and

(31)  $a$  exemplifies Redness

as logical grounds for concluding that  $a$  has a colour—i.e. our belief that (30) and (31) logically entail

(32)  $(\exists \Phi)(a$  exemplifies  $\Phi$  and  $\Phi$  is a colour)—

reveals our belief that 'Redness' has a common referent in (30) and (31), and hence that the term refers to the property Redness, not the class of red things, in (31). Now, it is incumbent upon anyone who wishes to hold that properties are meanings to specify just what *are* the expressions whose meanings properties are. In view of the present remarks and the similar conclusion from (i), above, properties cannot be the meanings of the singular

<sup>1</sup> A Church, "Abstract entities in semantic analysis", *Proceedings of the American Academy of Arts and Sciences* (1951), 80, 100-112.

forms of predicates. Hence if predicates *refer to* classes and *mean* properties, it must be the adjectival form of the predicate which has a property as its meaning. If so, it is incorrect to assume, as we have done here, that ' $x$  is red' has essentially the same force as ' $x$  exemplifies Redness', instead, we should consider the possibility that while the singular term 'Redness' means *Redness* and designates Redness, the adjective 'red' (or perhaps the full sentential function, ' $x$  is red') may mean Redness and designate the class of red things. But this seems most peculiar, for surely ' $x$  is red' is closer in meaning to ' $x$  exemplifies Redness' or ' $x$  has the property Redness' than it is to ' $x$  is a member of the class of red things'. Moreover, to say that 'red' (or ' $x$  is red') means Redness, constitutes a flagrant violation of the translation rule for filling the matrix, ' $\_$  means  $\_$ '.

It seems to me, therefore, that the reasons are overwhelming for concluding that if properties exist, they must be the referents of predicates rather than their meanings. Coupled with the interpretation of "existence" developed earlier, the ontological implications of this view are immediate and profound. Since whether or not a meaningful descriptive term has a referent is an empirical matter, *even if properties exist generically, the fact that we can construct a meaningful predicate from simpler expressions does not entail that there must correspondingly exist a property which is designated by that predicate.* Such considerations expose an acute need for sharpening a number of distinctions in the logic of properties which are frequently blurred. To illustrate this through a specific case, let us examine the compound predicate, ' $x$  is a red-square'. We may take

$$(33) \quad (\exists \Phi)[\Phi = (\lambda x)(Rx \cdot Sx)]$$

as an objective assertion that the abstract entity Red-squareness exists. Now, an existence assertion such as (33) must carefully be distinguished from one such as

$$(34) \quad (\exists x)(Rx \cdot Sx).$$

or, more explicitly,

$$(34') \quad (\exists x)[(\lambda y)(Ry)(x) \cdot (\lambda y)(Sy)(x)].$$

What (33) asserts is that Redness and Squareness are fused into a *single* property, whereas (34) merely claims that Redness and Squareness are co-exemplified. But it is unnecessary to assume that for every set of properties, there also exists an *additional* property which is somehow an amalgam of the set. There is nothing—or is there?—in the belief that certain abstract entities exist which commits one also to believe that they are endowed

with reproductive capacities. Moreover, both (33) and (34') must be distinguished in turn, from

$$(35) \quad (\exists \Phi)(x)(\Phi x \equiv Rx \cdot Sx).$$

While (34') implies the existence of no abstract entities other than Redness and Squareness, (35) asserts the existence of a third property which differs from both Redness and squareness in that it is exemplified by exactly those particulars which are both red and square. On the other hand, (35) is weaker than (33) in that a property in virtue of which (35) is true need not be identical with Red-squareness. Thus if all hard objects were both red and square, and conversely, (35) would be justified by the existence of Hardness. Finally, we must—or must we?—distinguish (33) from what appears to be an even stronger hypothesis,

$$(36) \quad (\exists \Phi)[(\Phi = (\lambda x)(Rx \cdot Sx)) \cdot (\exists x)\Phi x],$$

or

$$(36') \quad (\exists x)[(\lambda y)(Ry \cdot Sy)(x)],$$

which implies not only that Red-squareness exists, but also that it is exemplified.

It is important to note that although (35) appears to assert an ontological commitment beyond that implied by the descriptive use of 'Redness' and 'Squareness', most higher logical calculi would permit inference to (35) from the tautology

$$(37) \quad (x)(Rx \cdot Sx \equiv Rx \cdot Sx),$$

which is necessarily true if Redness and Squareness exist, by existential generalization over the compound predicate to the left of the biconditional. Similarly, it is customary to authorize inference of (36') from (34'). But if it is true that a meaningful compound predicate need not itself designate a property even though all its descriptive constituents have referents, then it is surely an ontological blunder to employ inference rules which construe *every* predicate, no matter how complex, as a descriptive term in the present sense. More generally, while it does not seem unreasonable to assume that a language user is committed to the existence of entities designated by the *primitive* extra-logical terms of all logical types in his approved vocabulary, it should be possible for him to assert non-atomic sentences without necessarily assuming any further ontological commitments (except, of course, to the existence of a fact corresponding to the asserted sentence.) Formally, this means that if the scope of the  $\lambda$ -operator is the criterion for the property-commitments of an assertion—i.e. if '...  $(\lambda x)(\Phi x)$  ...' is construed to entail '  $(\exists \Psi)[\dots \Psi \dots]$  '

—then postulates or inference rules by which the scope of the  $\lambda$ -operator may be widened should be regarded not as logical principles but as ontological assumptions which may, or may not, be justified by empirical reality. One need feel only the tamest platonistic yearnings to maintain that the assertion, 'There is something which is both red and square' entails commitment to the existence of Redness and Squareness, and even this can be questioned without denying the generic existence of abstract entities if it can be argued that 'red' or 'square' is not a primitive predicate. On the other hand, blithely to infer from 'There is something which exemplifies both Redness and Squareness' the further assertion 'There is something which exemplifies Red-squareness'—*i.e.* to assume that (34') in itself justifies (36')—is to leave oneself defenceless against both the importunities of the metaphysician and the ravages of the nominalist.

My point in all this is not to make any particular ontological contention, but to drive home the realization that once one sees clearly that not every meaningful predicate need be assumed to designate an abstract entity even when some may do so, one may not only assuage anti-metaphysical qualms about the generic existence of abstract entities, but may begin to ask a number of very interesting questions about such existences. I do not believe that such speculations are metaphysical gibberish. It seems to me that at the very least, questions about possible differences in the ontological force of, *e.g.* (33)-(36), find empirical significance in the rules of inference we are willing to adopt (*e.g.* existential generalization over compound predicates), which surely make a difference for the conclusions we draw from premises which we believe to be factually true. What would now seem to be called for is not breast-beating avowal of personal ontological faith, but meticulous study of alternate sets of ontological postulates to see exactly what testable differences they do make; or, if they *don't* make any testable difference, why don't they, and what, then, is the cognitive content of ontological speculation?

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### III.—ON THE GENERAL PROBLEM OF OBJECTIVE REALITY

BY PETER ZINKERNAGEL

ANY attempt to explain the meaning of the word "exist", when applied to ordinary things like tables and chairs in terms of observational concepts like "impressions", "sense-data" or "phenomena" leads to well-known paradoxes. It does so because the use of words like "tables" and "chairs" is logically dependent not on the use of words like "hear", "see", and "feel" but on the use of expressions like "what we can do" and "what we cannot do". It is not contrary to ordinary language to say that some material thing existed which we could not sense. It is contrary to ordinary language to say that a thing existed which did not in any way influence our possibilities of action. To say that things do not exist because they are not observed is therefore absurd.

In this paper I want to treat the problem of objective existence and I propose to do so by formulating three rules of language which to my mind have much the same sort of general validity as have the elementary laws of formal logic. I hasten, however, to stress the fact that the formulation is only tentative and does not by any means attain the precision characteristic of formal logic.

1. We must not use names of ordinary things and expressions for possibilities of action independently of each other.

2. We must not use psychological expressions independently of the personal pronouns.

3. We must not use the personal pronouns independently of designations for bodies.

The rules have deliberately been given a rather abstract and general form, but a few examples will, I hope, elucidate their content and show that we do in fact obey these rules when we use language for ordinary straightforward descriptions.

To illustrate the first rule of language let us see how we describe and how we do not describe such a well-known situation as a man sitting before a table on which an inkstand is standing. According to ordinary language we may say for instance that the man is able to move the inkstand and that by doing so he changes his possibilities of action. Having moved the inkstand there are now some things he can do which he could not do before moving it, and others which he cannot do but which he could do before moving it. Having moved the inkstand he cannot do for instance, to move it

across the place where the inkstand was standing, but he cannot move his hand freely across the place where it is now standing. and so on and so on.

If we want to be in accordance with ordinary language we would not say that he has moved the inkstand, but his possibilities of action are left unchanged so that he can move his hand freely across the place to which the inkstand was moved, or that he cannot freely move his hand across the place where the inkstand was standing. To say that he has acted (by moving an inkstand) without changing his possibilities of action would almost amount to saying that he has moved the inkstand without moving it, and would be in violent disaccord with ordinary language. In like manner it would be strange to talk about a man who was sitting before a table which did not confine his possibilities of action<sup>1</sup> or indeed to talk about a man who did not confine our own possibilities of action if we tried to move through him. Such a statement would seem to reduce the man to a most ghostly appearance. and it is certainly not in accordance with ordinary language to call ghosts ordinary things or people. I hope this is enough to show that we do not in fact use names of ordinary things and expressions for possibilities of action independently of each other. We arrive at most extraordinary statements as soon as we start to use the two sorts of expression independently of each other so that this rule may fairly be said to be a rule characteristic of ordinary language.

The second rule I shall illustrate by another well-known situation. namely that somebody has had a dream and wants to communicate his experience. It would be very much in accordance with ordinary language if he told us "last night I dreamt such and such a dream". It would not be in accordance with ordinary language if he told us "last night there was a dream but neither I nor anyone else had that dream" or "last night there was a dream which nobody had". And so with any other psychological experience which we want to talk about in a normal way : it would be in disaccordance with ordinary language to talk about a great joy which existed but was felt by nobody.

The third rule may be illustrated by the same example as the second rule. namely a man wanting to tell us that he has had a dream. He would be in accordance with ordinary language if he said "last night I was lying on my bed and I dreamt that I was

<sup>1</sup> The expression "to confine somebody's possibilities of action" does not belong to everyday language. It is only used as a short way of saying something which could be expressed in a series of statements about what we can and what we cannot do.

in the Sahara" or even "last night I was lying in my modest room and dreamt that I was a king". He would not be in accordance with ordinary language if he told us that he had had a dream without being either in his bed or in his room or at any other place. He would not be in accordance with ordinary language if he tried to use the word "I" independently of expressions referring to his own body and thereby referring to ordinary things. If he said for instance "I was thinking about some problem but I did not do my thinking either in my room or outside my room or anywhere else for I have not got a body". As names of ordinary things cannot be used independently of expressions for possibilities of action we are back at the first rule of language. It should be remarked, however, that the second and third rule are concerned not with the contents of psychological expressions but only with the formal conditions for describing such content. we might perhaps dream that we did not possess a body but if we wanted to describe our dream in a clear way we would still have to say "last night I lay in my bed and dreamt...".

I hope these examples are enough to show that we do in fact obey the formulated rules in ordinary straightforward descriptions. In my opinion they belong among the first rules we learn when we learn to talk and so should not be too difficult to recognize.

Before giving my reasons for saying that the rules possess the same sort of general validity as do the elementary laws of formal logic I should like to investigate what we mean when we ascribe general validity to formal logic. We do not mean that we ought always and under all circumstances to obey the laws of formal logic. As has been made abundantly clear by Wittgenstein and Ryle, language is used for many different purposes besides fact-stating and we would have to reject many of these uses if we demanded that the elementary laws of formal logic should always be observed. We would probably have to reject great parts of religion and of poetry. It would not be wise to condemn poetry because, *e.g.* the law of contradiction is sometimes broken. In like manner it would seem ridiculous to forbid jesting just because the law of the excluded middle should be strictly adhered to. We may even use formal contradictions to express difficulties of formulation as when we say "it is and it is not true that the law of contradiction should always be observed".

Notwithstanding all these qualifications I still take it that most of us would agree that we ought to avoid contradictions within a large class of descriptive and scientific statements. As far as I can see we have one important reason for holding this view.



namely that observance of the law of contradiction is an indispensable condition for unambiguous description. By breaking this law we may at once reduce any ordinary descriptive statement to nonsense. If we say "there is a bird sitting in the tree" and then add "and there is not a bird sitting in the tree" the statement becomes incomprehensible and could not be used for unambiguous description.

I shall argue that by breaking the proposed rules of language we may in a somewhat similar way reduce any ordinary descriptive statement to nonsense which could not be used for description. I should like, however, to stress once more that the formulation is much less precise than formal logic and that the reduction will be correspondingly less evident. The logical constants are often fairly easily separated and isolated from other words and so it is not too difficult to study their use independently of the use of all those other concepts together with which they are used in ordinary discourse. The words with which we are concerned in the three rules are much more intimately connected with other words and concepts and so it is much more difficult to isolate their use. There is one further difference between the proposed rules and formal logic, namely in the extent of their validity. While formal logic has general validity (probably because it is presupposed in the formulation of any other logical rule) within a class of statements stretching from the most commonplace utterance to the most subtle theorem in advanced mathematics, the three rules are not in any obvious way presupposed for instance in pure mathematics. Still, within a large class of statements which I have vaguely delimited by talking about straightforward descriptive statements I do think they have general validity. For many epistemological purposes this is enough.

Let us again consider the proposition "there is a bird sitting in the tree" and let us see what happens if we do not obey the first rule "there is a bird sitting in a tree which does not confine our possibilities of action and through which we may consequently move freely." "A bird which does not confine our possibilities of action sits in a tree which does not confine our possibilities of action." I do not think sentences like these are much more comprehensible than "there is a bird sitting in the tree and there is not a bird sitting in the tree". It would be almost as difficult to understand what situations were referred to in the one case as in the other.

At this point I should like to stress the fact that my arguments are arguments about logical rules, not about meanings or definitions in the traditional sense. Formerly answers to questions

about the meaning of words like "material objects" were often expected to take the form of an explicit definition, referring to common defining qualities and containing elements the meaning of which might be given through ostensive definitions. In my opinion some fundamental concepts ought to be treated in a manner somewhat analogous to the manner in which the logical constants are treated in a rule like the law of contradiction. I am thinking of the law of contradiction not as part of a deductive system but as a simple rule which we should obey if we want to give a straightforward description of anything. It seems to be typical for such a rule that it expresses logical relations between *different* concepts or words (here the words "not" and "both-and"), concepts which could not possibly be reduced to each other and the meaning of which could not be given independently of the logical relationships into which they enter. Any attempt to explain the meaning of a word like "not" in terms of say psychological concepts is bound to fail, because any psychological explanation or description presupposes the law of contradiction. The rule that names of ordinary things should not be used independently of expressions for possibilities of action does not mean that all material objects have in common the quality of being impenetrable. I do not think that the meaning of words like "material object" could be reduced to "impenetrability" or any other quality or combination of qualities but only that certain words should not be used independently of certain other words. The expression "possibilities of action" has been deliberately chosen because it is so obviously absurd to say that an ordinary thing like a table *is* a possibility of action or that a possibility of action *is* a table. I am not concerned with questions about what qualities should be demanded of an object to be rightly called a material object or with questions about our criteria for recognizing material objects, but rather with the conditions under which we may have such discussions. The point is admittedly a difficult one, but I may bring out the difference between the two sorts of questions by an example; we may discuss whether a cloud ought or ought not to be called a material object, and arguments might be brought forward on both sides. On the one hand it might be argued that clouds are penetrable and so ought not to be called material things, on the other that clouds are composed of molecules of the same kind as those of ice and that a clump of ice would certainly be called a material object. Now I suggest that during this whole discussion it is tacitly presupposed that names of ordinary things must not be used independently of expressions for possibilities of action.

We presuppose for instance that a cloud is something which may be reached if we get into an airplane and take off, that it may be found at a certain distance from the ground and so on. As far as I can see the discussion becomes nonsensical if we do not make that sort of presuppositions and we cannot express that sort of presuppositions in a clear way without adhering to the rule that names of ordinary things must not be used independently of expressions for possibilities of action. Therefore we might try to keep questions about logical rules apart from questions about defining qualities. We have to do with a logical rule when we can reduce to nonsense by breaking it. To carry through a reduction by means of the first rule it is not necessary to delimit the field of ordinary things. We need only use undoubted instances of such things.

Another example to illustrate the first rule "he was scolded by his chief for not having finished his work." "He was scolded by a chief who did not (literally) confine his possibilities of action." "He was scolded by his chief for not having done something which if he had done it would not (literally) have changed anything."

All three rules may be illustrated by transforming the same statement "he went through the sad experience of losing his dog." "A sad experience of losing a dog which did not confine anybody's possibilities of action was gone through without being gone through by anybody." "A sad experience was gone through by somebody who was not anywhere or who was somewhere without his body being there."

To get a clear impression of the indispensability of the rules one may try to describe such familiar situations as speeding along a highway in a car or looking at a beautiful sunset. One may try to use phrases like "we were speeding along a highway which did not confine our possibilities of action in a car which did not confine our possibilities of action". "To escape collision with another car which did not confine our possibilities of action we turned a steering-wheel which did not confine our possibilities of action."

"Standing on a mountain which did not confine our possibilities of action we were looking at a beautiful sunset." "We were looking at a sunset without being on a mountain or anywhere else." "We were looking at a sunset from a mountain, but our body was not on the mountain." "A beautiful sunset was seen but it was not seen by anybody."

I do not think the difficulties we would meet in trying to describe such familiar situations without adhering to the rules are

accidental. They would not diminish but grow with our efforts, just as any attempt to dispense with the law of contradiction would become more and more hopeless on closer inspection.

The indispensability of the rules becomes particularly evident if we investigate our conditions for describing psychological experiments. In ordinary language we describe a psychological experiment for instance by talking about placing a person before a dark screen with light spots and asking him to tell us what he sees. One may try to describe such an experiment un-ambiguously, without having recourse to the rules, by means of expressions like "we asked somebody to tell us what was seen without being seen by anybody", "we placed a person whose body did not confine our possibilities of action on a chair which did not confine our possibilities of action". How could we possibly repeat an experiment described in such language? Once more it should be noted that the rules do not in any way concern the contents of psychological experiences but only the formal conditions for describing them: we may devise an experiment which gives people the most extraordinary experience but we still have to describe the circumstances by obeying the most commonplace rules.

I hope this is enough to indicate the grave difficulties we would run into if we tried to dispense with the three rules in descriptions. A violation of them would make language almost as unfitted for description as would a violation of the elementary laws of formal logic. For ordinary descriptions we must therefore ascribe general validity to them just as we ascribe general validity to formal logic. Instead of saying that logical rules have general validity we might say that they represent indispensable conditions for clear and unambiguous description. Such a terminology would more immediately suggest the relevancy of logical rules to epistemology. If certain rules represent indispensable conditions for clear descriptions we ought to adhere to them if we want to give clear descriptions. And as epistemologists we ought to be interested in giving clear and unambiguous descriptions.

Recent discussions<sup>1</sup> show that there is some doubt about the status of, *e.g.* the law of contradiction and I should therefore like to elaborate the point.

Should somebody challenge the view that the law of contradiction is valid. I do not see how we could argue against him except by showing that by breaking this law we can immediately reduce to nonsense any statement he may care to produce. If this did not convince him we would be in a rather helpless position.

<sup>1</sup> See C. A. Campbell, "Contradiction. 'Law' or 'Convention'", *Analysis*, March 1958.

We could of course point out that one consequence of his view would be that his view might be both right and wrong (not right). If, however, this did not deter him, if he said that he did in fact understand (and not understand) a formal contradiction and that contradictions both could and could not be used to give clear descriptions, we would have to acknowledge certain defeat. We could not even evaluate his position as right or wrong for we could not understand his use of such words (or for that matter his use of the word "understand").

As far as I can see we could only have one reason, but a conclusive one, for saying that adherence to the law of contradiction is an indispensable condition for unambiguous descriptions, namely that we cannot in fact dispense with it. There could only be one reason, but a conclusive one, for saying that adherence to the law is not an indispensable condition for descriptions, namely that we could in fact dispense with it and still give unambiguous descriptions. In like manner there could be only one reason for or against the indispensability of the proposed rules, namely that they could or could not be dispensed with in unambiguous descriptions.

If this is so then it follows that all other arguments for or against their indispensability must be illusive. Let me give a few examples of other arguments which might be thought relevant.

Until quite recently a great part of the philosophical world was—as regards the relationship between language and reality—dominated by a view which might be called the theory of analytical and synthetical propositions. According to this view all propositions may be divided into two classes, the synthetical propositions and the analytical propositions. The synthetical propositions may be true or false according to their correspondence with facts, while the analytical propositions are always true because they do not deal with reality but only with conventional linguistic rules. Now if this theory were correct the three rules would have to belong to one or the other of the two classes. As they can hardly be said to describe facts in any usual sense (in my opinion they represent conditions for describing any particular fact) they must belong to the analytical propositions and consequently they must be arbitrary. But to say that they are arbitrary is the same as to say that they are not indispensable. To say that they are indispensable but nevertheless arbitrary is to use the word "arbitrary" in a most confusing way. So if we accept the theory of analytical and synthetical propositions we must also accept that the three rules are not indispensable conditions for description. But do we have to accept the theory?

Few people would deny that it may sometimes be useful to distinguish between what is true by arbitrary definition and what is true because it corresponds to matters of fact. It seems, however, rash indeed to infer from this that *all* true propositions must belong to one or the other of the two categories. If the theory of analytical and synthetical propositions is correct, then the proposed rules are not indispensable conditions for description. If, on the other hand, the rules are indispensable then the theory of analytical and synthetical propositions is wrong, considered as a universal theory about the relationship between language and reality. How should we decide the issue if not by investigating the facts in the case, namely whether we can or cannot dispense with the rules and still talk intelligibly?

A similar argument runs like this: how do we know that the so-called indispensable rules of language are not particularly deep-rooted habits of speech, that their apparent necessity does not derive from their use during countless ages, that they would not or could not have been different if our habits of speech had been different? In support much evidence might be adduced tending to show that certain modes of speech, by the users felt to be both natural and necessary, were in fact different among people belonging to a different family of language. My answer would be that it would lead to nonsense to say, *e.g.* that the law of contradiction is only a habit of speech. The law of contradiction is an unavoidable condition for the use of any expression including the expression "habit of speech". We could only explain what is meant by "habit of speech" by adhering to the law of contradiction. If, having given such an explanation, we added "and the law of contradiction is only a habit of speech", we would be saying something very strange. We would be saying that we could only explain the meaning of "habit of speech" by adhering to a certain law and adding that this law was itself an example of that which could only be explained by accepting its validity. Should anybody be able to understand the meaning of such a statement? Could it by any standard of unambiguity be called unambiguous? In like manner I think we can explain what is meant by "habit of speech" if we observe the proposed rules and talk about people and their circumstances in the usual way. But I think we should be led into grave difficulties if we attempted to explain what is meant by "people" without adhering to these rules, let alone explaining what is meant by their habits of speech. If we can only talk about the meaning of "habits of speech" under certain conditions it is not illuminating to call these conditions habits of speech.

Similar difficulties arise if we call logical rules mere laws of thought or say that they are only psychologically valid. If there are certain conditions for giving any psychological description we cannot without nonsense call them psychological.

The examples suffice, I hope, to show that if we want to investigate our conditions for unambiguous description we had better do so and not start with any preconceived ideas about the relationship between language and reality or about the nature of logic or even about the number of logical rules.

After these preparations the problem of objective reality will give us little difficulty. Since Descartes and Berkeley discussions have mostly proceeded on the assumption that we have something given in consciousness and should try to infer from this the existence of something existing independently of consciousness. Put in another way it was assumed that primarily we need "psychological" expressions ("think", "feel", "see", "hear" and so on) to describe our experiences.

Now if the rules are valid the assumption is simply nonsensical. According to the rules psychological expressions must be used in connection with personal pronouns (or of course persons' names) and these in connection with designations for bodies which in this respect may be treated like names of ordinary things.<sup>1</sup> That is we must not use designations for bodies independently of expressions for possibilities of action. It is not so that the existence of ordinary things must be inferred from phenomena in consciousness. It is on the contrary so that the description of any content of consciousness presupposes a certain use of names of ordinary things like tables and chairs. According to the rules we cannot use even the most "purely psychological" expressions like "joy" or "anger" without referring to things which exist independently of consciousness. A proposition like "things do not exist when they are not perceived" is contrary to the rules because it makes the use of the word "exist", as applied to ordinary things dependent on a psychological word like "perceive", while the rules say it is the other way round. We must not use a word like "perceive" without using personal pronouns and names of ordinary things in connection with expressions for possibilities of action. Psychological descriptions are irrelevant for objective existence because objective existence must be presupposed by any psychological description.

<sup>1</sup> This does not imply that living bodies must be considered as mechanical systems. I believe on the contrary that it leads to contradiction to say that we might give a completely deterministic description of an organism.

To start epistemological analysis with the assumption that something is given in consciousness which may be described without referring to anything else is like starting with the assumption that we can understand a formal contradiction. To go on without revising the assumption is like seeing what would follow if a formal contradiction was allowed. As is well known anything follows if we allow formal contradictions and so the strange consequences of many philosophical writings ought not to surprise us. Unfortunately it is much easier to overlook the logical conditions for the use of psychological expressions than it is to overlook the logical conditions for the use of expressions like "not" and "both-and". In some cases it is even explicitly stated that there are no logical conditions for the use of a word like "consciousness" except those expressed in formal logic. I refer to expositions where only explicit and ostensive definitions are recognized. *A priori* we cannot know how many logical rules there are besides those governing the use of the logical constants, but if we decree that none could be found we are not likely to find any. And if we do not find any we may say almost what we please if only formal logic is not violated. As long as we may say whatever pleases us we cannot hope to create an objective science.

Since the advent of Ryle's book, *The Concept of Mind* philosophical discussions have undergone a profound change. Attention has been drawn to what is called the informal logic of ordinary language and a more sober attitude has prevailed. The importance of language to epistemology should be evident as soon as we reflect that we have to use language in any philosophical exposition. So we had better take care that we do not misuse it. As is well known the so-called Oxford school has been mainly criticized for reducing philosophy to linguistics. I think the criticism is mostly beside the point, but I do not think that Oxford philosophers have said quite clearly why it is so. To be sure Ryle has emphasized the fact that we are as much concerned with concepts as with words and are interested not in linguistics but in logic.<sup>1</sup> If, however, we try to explain the difference between logic and linguistics by referring to some standard use we are apt to get rather involved in what we say. In an answer to the criticism that Oxford philosophers are primarily interested in the justification of ordinary language Morris Weitz writes: <sup>2</sup>

<sup>1</sup> See for instance Gilbert Ryle, "Ordinary Language", *Philosophical Review*, 1953.

<sup>2</sup> Morris Weitz, "Oxford Philosophy", *Philosophical Review*, 1953.



What is being justified, defended, or legislated is not an ordinary expression but the rejection of a way of talking on the ground that a claim is made that it is sanctioned by the standard use of an expression when, as a matter of fact, the standard, actual use does not sanction that way of talking. What is being said by these philosophers is not that the standard use is inviolable, the logic of an expression, while it does not fluctuate like expressions themselves, does change, but what is central: what can and what cannot be said in terms of a particular standard use.

If there are certain indispensable conditions for unambiguous description we can, at least as regards the descriptive use of language, perhaps say a little more clearly why and how we distinguish between logic and syntax: we are concerned with logic when we investigate our conditions for description. By an indispensable condition for description we will understand a rule the violation of which makes unambiguous description impossible. In the above mentioned paper Ryle says that philosophers are interested in what is common to different languages. If there are certain indispensable conditions for description we have reason to believe that such conditions are the same in different tongues.

An interesting fact emerges, if it be accepted that the proposed rules have general validity, namely that we can only describe ordinary experiences in a clear way by obeying some abstract logical rules. Although there are certain indispensable conditions for describing experience, we cannot possibly explain why it is so. We cannot analyse the relationship between experience and description and explain because experience in such and such description must be such and such. We cannot do this for the simple reason that any explanation, if it were well-defined, would have to obey the very rules which it was intended to explain. Any concept which might be used in the explanation depends on the rules and could not be used to explain them. If it is true that we can only describe a certain content by using a certain form, then we have no means of analysing the relationship between that particular content and that particular form. The functioning of logical rules may be exemplified and we must verify that they are in fact indispensable, but they could never be explained.

Disagreement between positivists and Oxford philosophers centers round different conceptions of language. The positivist conception goes in the main back to Berkeley and Hume and decrees roughly that only such elements have a legitimate function in language to which corresponding elements could be

found in reality. The theory was violently criticized in Ryle's review of Carnap's book, *Meaning and Necessity*.<sup>1</sup> Oxford philosophers on the other hand put stress not so much on the elements of language as on the rules governing the use of the elements. Positivists look on language somewhat in analogy with a picture of reality while Oxford philosophers tend to regard language more in analogy with a game. In my opinion there can be little doubt that the game analogy is the more fruitful and it corresponds to trends within modern linguistics.<sup>2</sup> Nevertheless it must be borne in mind that language must be considered as *sun generis* and that accordingly no analogy could ever be quite satisfactory. If we confine ourselves to what has vaguely been called descriptions, the uniqueness of language reveals itself in the fact that we can only distinguish between logic and syntax by referring to the descriptive function of language. The game analogy breaks down before the descriptive function of language just as the picture analogy breaks down before the fact that language can only fulfil its descriptive function if certain abstract rules are observed

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<sup>1</sup> Gilbert Ryle, "Meaning and Necessity", *Philosophy*, 1949.

<sup>2</sup> It is interesting to note the likeness—as regards essential features—between modern physics, linguistics and epistemology. See for instance Niels Bohr *Discussion with Einstein*, and Louis Hjelmslev, *Prolegomena to a Theory of Language*

## IV.—TWO TYPES OF HYPOTHETICAL STATEMENTS

BY F. J. CLENDINNEN

In this paper I want to draw a distinction between two kinds of hypothetical statements, and to show that the relation between an argument and a hypothetical statement of the one sort is different from that between the argument and the hypothetical statement of the other sort.

A hypothetical statement may assert a connection between two actual statements: for example, "If Jones has passed his examinations, his appointment is certain", and "If all metals are conductors, then copper is a conductor". Such hypotheticals we will designate "concrete".

There are also hypotheticals which include in their clauses a variable so that a relationship is asserted not between an actual pair of statements but between any pair of statements which results when the variables are given values. For instance, "If anything is a metal it will conduct electricity" asserts a relationship between all statements of the form "It is a metal" and those of the form "It conducts electricity". Such hypotheticals we will call "variable".

Since according to the normal convention for the use of symbols in logic, "p" and "q" stand for propositions or statements, hypotheticals represented by "if p then q" will be concrete hypotheticals. Those represented by " $(x)$  if  $Fx$  then  $Gx$ " will be variable hypotheticals.

Any argument at all can be characterised as "p so q", for even if the argument is quite complicated we can take the conjunction of all the premisses together as the premiss "p". Given an argument "p so q" there is one concrete hypothetical which corresponds to it uniquely, namely "if p then q". We will call the hypothetical standing in such a relation to an argument the "*ad hoc* hypothetical of the argument".

In general there will be no single variable hypothetical that corresponds to an argument. In constructing the *ad hoc* hypothetical of an argument we make direct use of the statements that constitute premiss and conclusion of the argument. But if we set out to construct a variable hypothetical we must construct a statement frame from a statement, and if there is more than one term which could be replaced by a variable there will be no single way of doing this. For example, from the statement "This

cylindrical piece of copper is an electrical conductor", we could construct such statement frames as "X is a conductor", "X is a copper conductor", "X is a cylindrically shaped conductor", etc.

We must note the existence of some hypotheticals which are ambiguous as regards their classification as concrete or variable Words such as "this", "that" and "today", which in their primary meaning refer to something indicated by the context, can also be used as variables, especially in hypotheticals. "If this is an A it is a B" could refer only to the single object, but on other occasions it could be used with the same meaning as "If anything is an A then it is a B". "If today is Monday then tomorrow is Tuesday" could well be used with the same meaning as "If any day is a Monday then the following day is a Tuesday".

From our general knowledge and the context it will usually be clear when one of these ambiguous locutions is being used exclusively as a concrete hypothetical. However when the variable hypothetical is clearly true the question "which meaning holds?" might be unanswerable, for the ambiguity in the expression might be paralleled by an ambiguity in the intention of the speaker.

For example, a person saying "If today is Monday then tomorrow is Tuesday" may be concerned solely with the day following the day on which he is speaking. To this extent he intends the concrete meaning of the hypothetical sentence. Yet he certainly knows that if any day is a Monday then the following day is a Tuesday, even assuming that he has not deliberated on it before speaking. There would seem to be a case both for and against saying that he intends the variable as well as the concrete meaning of his locution. I do not want to go into this question here, but merely to distinguish the two distinct roles that such sentences can play.

In his article "If, So and Because",<sup>1</sup> Professor Ryle deals, among other things, with the question: "How does an argument require the truth of the hypothetical statement which corresponds to it?" Since an argument is not a statement, the argument can not entail the hypothetical statement nor can the argument in any sense include the hypothetical statement. Nor is the view that the hypothetical is just the argument reworded tenable, for the hypothetical does not assert that which is asserted by the premiss and conclusion in the argument. Again, the view that the hypothetical is required by the argument to make it valid leads to an infinite regress.

<sup>1</sup> G. Ryle, "If, So and Because", *Philosophical Analysis*, ed. M. Black, p. 323

According to Ryle, hypothetical statements must be seen as "post inferential levels of discourse" or "sophistications upon inference". That is, to understand a hypothetical statement we must know what it is to infer or argue, for in learning "if  $p$  then  $q$ " I am learning that I am authorised to argue " $p$  so  $q$ ".

In dealing with this relationship between arguments and hypothetical statements it is important to consider the difference between variable and concrete hypotheticals. Ryle's position as applied to concrete hypotheticals needs no qualification. To say that the argument " $p$  so  $q$ " is valid is exactly the same as saying that the *ad hoc* hypothetical "if  $p$  then  $q$ " is true.

However the case with variable hypothetical is not the same. We can agree that a variable hypothetical says primarily that a certain sort of argument is valid. However, in saying this something else is implied. "If anything is an  $A$  then it is a  $B$ " asserts primarily that we can always conclude a statement of the form " $X$  is a  $B$ " from a statement of the form " $X$  is an  $A$ ". For this to be so there must be a certain pattern in the facts, the pattern that is asserted by "all  $A$ s are  $B$ s".

No such factual claim is made by concrete hypotheticals. Whether the argument "this is an  $A$  so it is a  $B$ " is valid certainly depends on what the facts are. But the statement that this argument is valid does not tell us what these facts are. For there are various factual patterns that could justify the argument. The argument will be valid if all things that are  $A$  are  $B$ . It will also be valid if only those things which are both  $A$  and  $C$  are  $B$  and if it had been noted, although not mentioned, that this is a  $C$  as well as an  $A$ . There might be, in fact, quite a number of features of a context taken into consideration when we say "If this is an  $A$  then it is a  $B$ ". For example, in the light of the information that Tuesday 10th June was about seven days ago, the argument "Today is Monday so tomorrow is the 17th June" is valid. The validity of this argument does not imply the truth of "If any day is a Monday then the following day is the 17th June". Again, even if it was false that all metals are electrical conductors it would be possible for the argument, "this is a metal so it is an electrical conductor", to be valid. It would be valid if it had previously been established, perhaps on the grounds of its colour, that the only metal the specimen could be was copper which was known to be an electrical conductor.

The *ad hoc* hypothetical "if this is an  $A$  then it is a  $B$ " tells us only that the argument is valid. It gives us no information about why the argument is valid, because it does not tell us what pattern in the facts is responsible for the validity of the argument. The

variable hypothetical, on the other hand, does tell us why the argument is valid, because it asserts, implicitly, that there is a specific factual pattern and it is this pattern which makes the argument valid.

Ryle says that the principle of an inference cannot be one of its premisses. He goes on. "It is not merely that the officially recognised Rules of Inference cannot be given the role of premiss components in all the specific inferences that are made in accordance with them. The same thing is true of the most 'meaty' and determinate hypothetical statements, like 'if today is Monday, tomorrow is Tuesday' . . . . The principle of an inference cannot be one of its premisses or a part of its premiss."<sup>1</sup>

Now certainly in calling something a principle of an argument we are distinguishing it from the premiss. But something more than a verbal point is at stake here. Is there a categorical difference between the premiss and the principle of an argument, or might the principle have been stated as part of the premiss, if, for example, the argument had been more fully stated? Let us consider separately concrete hypotheticals, rules of inference and variable hypotheticals.

The *ad hoc* hypothetical of an argument cannot be described as a suppressed premiss of the argument, for the argument "P and, if P then q, so q" includes a redundancy. Just as much is said by the original argument, "P so q". All that has been added is the assertion that the original argument was valid. That this was believed by the arguer was evident from the fact that he so argued, and consequently there is no point in his stating it. Certainly its addition in no way strengthens or expands the argument.

A rule of inference would also be redundant if added to an argument, although in a rather different way. A rule of inference only applies to an argument which is already logically valid, that is an argument which can be seen to be valid without recourse to any additional information. The addition of anything to such an argument is clearly redundant, especially if that which is added can be seen to be true with no more ease than the argument can be seen to be valid.

To restate the original argument so as to include the variable hypothetical would not be to add something redundant, for the variable hypothetical not only asserts that the original argument was sound, but also states why, as it gives additional information.

*Ad hoc* hypotheticals and rules of inference are, then, categorically different from premisses, but variable hypotheticals are not.

<sup>1</sup> Ryle, *op. cit.* p. 328

In many cases the addition of a variable hypothetical, or the generalisation that it implies, to an argument would be pointless simply because its truth is so well known. But this does not constitute logical redundancy. To sum up, arguments of the form "p, if p then q, so q" include a redundant premiss, but arguments of the form "(x) if Fx then Gx, Fa so Ga" do not.

Ryle proposes the term "inference warrant" to designate hypotheticals and also to indicate their relationship to arguments. He includes both concrete and variable hypotheticals under the term. This concept of "inference license" seems to rest on two separate features of hypothetical statements, the categorical distinction between hypotheticals and premisses, and the role of hypotheticals as giving the justification of arguments. But no hypothetical has both these features. Particular hypotheticals are categorically different from the premisses, but do not give the grounds of arguments. Variable hypotheticals do give the grounds of arguments and might be called inference warrants. However these are not categorically different from premisses and might well be called suppressed premisses.

I now want to show how the distinction between the two kinds of hypotheticals is necessary to deal with quite a different problem—namely, to distinguish those arguments which are demonstrative in their logical form from those which are not.

In dealing with arguments as actually uttered, the distinction between demonstrative and non-demonstrative can be drawn with precision. A "demonstrative argument" can be defined as one in which a contradiction results if the premiss or premisses are asserted and the conclusion denied. Faced with an actual argument we can apply this test in a quite straightforward manner, as there is not likely to be much difficulty in deciding whether a given group of statements constitute a self-contradiction.

However, we cannot rest content with this distinction. In the first place hardly any arguments as actually stated are demonstrative on this definition. Even arguments in geometry, such as "In triangle ABC side AB is equal to the side AC so angles B and C are equal", or proofs as given by Archimedes, for example, would not be demonstrative, for no contradiction would be involved in asserting the premisses and denying the conclusions. Secondly, the fact that many such arguments are not demonstrative is purely accidental as far as logic is concerned. Consider these two arguments: "All sixth form boys are sitting for their exams this term so John is", and, "All sixth form boys are sitting for their exams this term and John is a sixth form boy so

John is sitting for his exams " The latter is demonstrative ; the first is not. Yet both arguments express the same inference ; the first is, as it were, an abbreviation of the second Similarly, arguments in geometry could be expanded so that the theorems on which they depend were actually stated instead of being taken for granted

If the distinction between demonstrative and non-demonstrative is going to reflect a logical and not purely stylistic distinction, it would seem that an argument should be fully stated before the test is applied However, unless care is taken in specifying what is to count as a fully stated argument, this policy in its turn can lead to paradoxical results. Arguments which have been traditionally represented as the opposite of demonstrative may appear to be demonstrative.

To state an argument fully, it is, presumably, necessary to make explicit any suppressed premisses If *ad hoc* hypotheticals were accepted as suppressed premisses, all arguments would be demonstrative when fully stated. However we have seen that it is inappropriate to consider the *ad hoc* hypothetical of an argument as a suppressed premiss An *ad hoc* hypothetical simply asserts that the argument is valid. While a variable hypothetical could give information additional to that in the premiss of an argument, a concrete hypothetical could not, and its addition to the argument would be redundant.

Again, to suppose that the *ad hoc* hypothetical was a suppressed premiss of an argument which must be stated to give the argument in full, would lead to the infinite regress that Ryle has indicated Once the *ad hoc* hypothetical of an argument has been added to its premiss we have another argument with a more complex premiss, and this argument in turn has an *ad hoc* hypothetical which must be stated, and so on to infinity.

Thus the fact that there is a sense in which the *ad hoc* hypothetical is presupposed by an argument must not lead us to consider it as a suppressed premiss A suppressed premiss should be seen as a statement of information which is presupposed in uttering the argument. As *ad hoc* hypotheticals assert no new information they are not suppressed premisses

We now have a definition of demonstrative arguments, namely, those in which a contradiction arises when the stated and suppressed premisses are asserted and the conclusion denied This definition depends on that of "suppressed premiss" given above. But difficulties could arise in applying this latter definition, for it might be doubtful whether certain statements are presupposed. This is an inevitable consequence of going beyond that which is



actually given in an argument. However, when it is uncertain which of two or more alternatives is presupposed by an argument, we can decide the logical structure of the argument for each alternative.

Contemporary logicians who claim that inductive arguments become demonstrative when all the premisses are stated,<sup>1</sup> do so by allowing *ad hoc* hypothetical as premisses. Suppose we have argued "all As investigated are Bs, therefore all As are Bs". Now it is suggested that in so doing we are assuming that that which is common to the sample is common to all individuals of the given kind and that if this assumption is stated as an additional premiss of the argument it becomes demonstrative. But this assumption is somewhat ambiguous and we will find that if it is clarified it is either the *ad hoc* hypothetical or alternatively a statement which is clearly false and which would not normally be presupposed by anyone arguing inductively. "What is common to the sample" might refer to the particular property in question, in this case the assumption becomes equivalent to the *ad hoc* hypothetical, "If this property is common to this sample, it is common to all individuals of this kind". On the other hand, "what is common to the sample" might mean "whatever is common to the sample". On this interpretation that which is suggested as the assumption of the argument is patently false, for given any sample at all, there must be some properties which are common to it but do not extend to the class as a whole. If no other, there is the property of belonging to the sample, and certain spatial and temporal properties associated with this.

When we argue from a sample to all individuals of the kind there is almost certain to be additional information either explicitly stated or presupposed. Usually such information will only strengthen the argument, but in some cases, when the additional information takes the form of a suitable generalisation, the fully stated argument will be demonstrative. I do not want to claim that everything which appears to be an inductive argument is non-demonstrative, but merely to reject the view that for every inductive argument there is a suppressed premiss which, if stated, makes the argument demonstrative.

This view is only tenable if we neglect the important distinction between the way some arguments presuppose additional information, and the way in which all arguments "presuppose" their *ad hoc* hypothetical.

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<sup>1</sup> See, for example, Cohen and Nagel, *An Introduction to Logic and Scientific Method*, p. 276

## V.—THE "IS-UGHT": AN UNNECESSARY DUALISM

By M. ZIMMERMAN

SUPPOSE we never break through the "is-ought" barrier, what then? Let us speculate. Then we can never justify ethics and morality. Well, perhaps this would only be true for naturalists, empiricists, emotivists, *i.e.* for those who believe that statements are justified only if supported by "is" statements. Hold on. How could they talk about statements being "justified" only if supported by "is" statements, if by "justified" they mean "ought to be believed"? Would not this be one case of an "ought" statement in the need of being justified by being supported by "is" statements, one alleged break through the "is-ought" barrier?

Well, let us go on. Suppose everybody took this position, *i.e.* nobody ever believes anything not supported by "is" statements. Also, suppose by "ethics" and "morality" we mean "ought" statements. Then it would follow that we could never justify ethics and morality. Remember, nobody believes anything not supported by "is" statements, we can never get "ought" statements from "is" statements (we cannot break through the "is-ought" barrier), and ethics and morality consist of "ought" statements.

Disaster! We can never justify ethics and morality? What have they done to us? Well, let us continue speculating.

Suppose a prisoner, who recently killed his wife and three children admittedly to collect their life insurance, and is found sane, is before a judge about to sentence him. Ought the judge to sentence him? Remember, neither we nor the judge believe we "ought" to do anything since we cannot get an "ought" from an "is" and we only believe statements supported by "is" statements. Well, the judge wants to sentence him. We want the judge to sentence him. We believe the judge will sentence him because we believe the judge wants to remain a judge and will not remain a judge if he does not sentence him. These statements can be supported by "is" statements, so we *can* believe them. Well, "ought" the judge to want to sentence him, "ought" we to want him to sentence him? Of course not, since we do not believe "ought" statements. Does that mean the judge will not want to sentence him, that we will not want the judge to sentence him? of course not, since these are all "is

supportable" statements Well, how do we know the judge wants to and will sentence him, that we want the judge to sentence him? Well, these are "is supportable" statements and we may know them from what we know about judges who want to remain judges, about people, psychology, law, government, crimes, punishment, etc. These are all "is supportable" statements.

Well, let us revise our story Suppose the prisoner turns out to be insane, completely insane, has been for a long time Then we ought not to punish him; confine him, institutionalize him, treat him, yes, but ought we to punish him? No. But hold on, we cannot say we "ought" not to, remember? Well, will we refrain from punishing him, will the judge refrain from sentencing him, knowing that he is insane? That depends.

Let us digress for a moment What would happen in a society of "ought" believers, would they punish the insane man? Well, that depends. Some people would believe we "ought" to, and some we "ought" not to.

Let us return to the society of "is" believers. Would they punish the insane man, would the judge sentence him? Well, some of us would want to punish him even if he is insane. Some of us, however, would not want to punish him, would feel nothing is to be gained by it, that more is to be gained by institutionalizing him, treating him, etc. Remember, these are all "is supportable" statements, and we can believe them and know them the way we know any kind of an "is supportable" statement.

True, but can we show the people who want to punish the insane man that they "ought" not to punish an insane man? Of course not. "Ought" statements, *verboden*, remember? Well, can we show them that nothing is to be gained by punishing him, that institutionalizing him, etc., will prevent him from committing similar crimes? Surely we can the way we can for any "is supportable" statement.

Yes, but will we show them that nothing is to be gained from punishing him, etc., will we want to do this? Well, some of us will, some of us will feel sorry for the insane man, will sympathize with him, because he could not help doing what he did. Hold on. How is it that because he could not help doing what he did, we will want him not to be punished, we will want to persuade others not to want him to be punished, etc.? After all, we cannot say that because he could not help doing what he did, he ought not to be punished, that we ought not to want him to be punished, can we?

Sure, but it is a fact that some of us will not want him to be punished for something he could not help doing, this is an "is

supportable" statement. It is also a fact that some of us will want to and try to persuade others to feel and want and act in the same way. These are all "is supportable" statements.

Well, but this is all pretty arbitrary, is it not? After all, people's feelings are pretty fickle, liable to change from one moment to the next. Thus, as originally supposed, if the prisoner were sane, his being punished would depend on how the judge, on how we happened to feel at the time. Likewise, if the prisoner happened to be insane, his fate would be subjected to the same sort of arbitrariness would it not? Well what do you expect in a society without "ethics" and "morality"? It would be no surprise if a sane man got away with murder and an insane man were to be "murdered".

Let us digress again. Would it be any different in a society of "ought" believers? Would not there be some who felt we "ought" to punish a man even if he were insane? Would anything be gained by saying that something ought to be done or not done merely because it just ought to be done or not done? Would any more be gained in such a society by trying to support "ought" statements with "is" statements than in the society we have imagined in which "ought" statements were dispensed with and only "is" statements were used?

But still, in a society of "ought" believers we have "ethical" and "moral" standards to appeal to, whereas in a society of "is" believers there are no ethical and moral standards as guides, so does not this make a big difference?

Well, but in a society of "ought" believers, where there are disagreements in ethical and moral standards what is there to appeal to? Is it not merely a matter of some saying we ought because we ought and others saying we ought not because we ought not? Is not this arbitrary? And if we appeal to "is" statements in support of "ought" statements, are we any better off than a society of "is" believers, with the added disadvantage of trying to break through the "is-ought" barrier? How frustrating!

But look here, what about the areas of agreement in ethical and moral standards, not only among different cultures and societies, but even where they disagree, within different cultures and societies? Do not ethical and moral standards serve a useful and essential function here? For example, should not we be thankful that in Western cultures there is fairly universal agreement (even where we sometimes pay mere lip-service to it) that we "ought" not to punish an innocent person and we ought not to punish even a "guilty" but insane man?

However, is not the point not that we generally agree that we ought not to do these things, but rather that we do not do these things, we do not want to do these things, we want to try to persuade others not to want or do these things, and we set up laws and courts to prevent these things from occurring? Are not these all "is" and "is supportable" statements which would be just as true and to the same extent believed in a society of "is" believers? Do not people generally try to do or prevent what they want very much to do or prevent, and would not a society of "is" believers want and try to do or prevent what a society of "ought" believers believe they ought to do or prevent?

Have we not overlooked an important function of "ought" statements which is brought out by the fact that we do not normally, it would be odd to, tell people that they "ought" to do something they want to do? Is it not the getting people to do what they do not want to do or refrain from doing what they want to do, by telling them they ought or ought not to respectively, precisely what would be lacking in a society of "is" believers?

Yes, but can you think of any case where we get people to do what they ought to though they do not want to, merely because we tell them they ought to, where we could not have found a better way, without use of "ought" statements, to achieve the same end? Let us look at this more closely

If a man wants to break promises, tell lies, rape or kill, which is better, merely telling him he ought not to, even if it succeeds in restraining him, or telling him that if he does what he wants, he will be disliked, ostracized, punished or killed? This is not all. We can not only tell him these things, we can do some or all of these things. But there is even more, much more and even more important. We can use all our resources of knowledge, in the sciences, in psychology, economics, sociology, etc., and the further acquisition of knowledge to get him and others to do the things we want him and others to do. Note that these are all "is" or "is supportable" statements. Incidentally, is not this part of what Socrates was getting at when he said that evil is a result of ignorance?

Well, but have not we completely disregarded those who believe we can begin with "ought" statements, that we do not have to get them logically, deductively or inductively, from "is" statements, that by "intuition" or "insight" or "non-natural" qualities, we can know what ought or ought not to be? Furthermore, even if we cannot get from an "is" to an "ought" surely this is not a symmetrical relation, surely we can get from an

"ought" to an "is"; so if those who believe we can begin with "ought" statements are correct, would not dispensing with ought statements be doing away with an important body of ethical and moral truths? If we can know and let others know, we ought to tell the truth, to keep promises, to preserve life, not to commit adultery, not to inflict needless pain, and knowing this we can infer that we are more or less likely to *do* what we ought or ought not to, would not discarding "ought" statements be sheer folly, even madness?

Yes, but even if those who believe we can begin with "ought" statements were correct, even if we could know these things, is not the important thing not that we could know these things, but what we would do and refrain from doing? Is it really the case that if we could really "know" that we ought to do something, that if we were to nevertheless dispense with believing "ought" statements, that we are less likely to do it? Even if we dispensed with "ought" statements, is it not true that we would tend to want to do what we happen to believe we ought to even if we also had "is" reasons for not wanting to do it? Even in those cases where we have an overwhelming desire not to do what we believe we ought to, either the use of "ought" statements will be ineffective, or, as already indicated, use of or appeal to other means (punishment, education, etc.) will be as likely (if not more) to accomplish the end.

More important than this, it is as difficult, if not more, to show that we can begin with "ought" statements as it is to break through the is-ought barrier. Worse, those who believe we can, have continually disagreed over what ought statements to begin with. If we are told by some that it is self-evident that we ought to preserve our own life, and by others that it is self-evident that we ought to sacrifice our lives for our country, what then? If two incompatible statements are said to be self-evident and it is really the case that one of them is self-evident, then it follows that the other one cannot be, but has been mistakenly taken to be self-evident. It does not even make sense to ask, how do we decide, for deciding implies looking for evidence which implies that they are not self-evident to begin with. And even if we look for evidence, *i.e.*, evidence consisting of "is supportable" statements, we are back to the "is-ought" impasse.

But you know, we have been supposing all along that we cannot break through the is-ought barrier. Suppose we can, suppose those who believe we can are correct? Now, I do not have in mind those who believe we can because they believe "ought" statements are translatable into "is" statements, in the last

analysis, this comes to dispensing with "ought" statements anyway for it amounts to believing "is" or "is supportable" statements only, whether in "disguised" form or otherwise. I am referring to those who believe that, just as "is" statements have a "logic of their own", inductive or deductive, which enables us to give acceptable "is" reasons in support of them, "ought" statements have their own kind of logic which permits us to give acceptable "is" reasons in support of them.

For example, it seems reasonable to accept as an "is" reason for saying we ought not to punish an insane man, that he could not help doing what he did, does it not? Suppose this were true for other "ought" statements as well, suppose that, at least in many other cases, we could break through the is-ought barrier, would not dispensing with "ought" statements be like dispensing with inductive arguments because it has a "different kind of logic" from that of deductive arguments?

Well, suppose in the case of some "ought" statements we can break through the is-ought barrier, suppose, for example, that most people find the "is" statement that a man could not help doing what he did, an acceptable "is" reason for saying that he "ought" not to be punished? What are we aiming for here in getting people to say that an insane man ought not to be punished, if other than that we do not want to and will not punish an insane man? Would it be any different if we had said that since he could not help doing what he did, we have an acceptable reason for saying that we do not want, we do not want others to want, we will not and will try to persuade others not to, punish him? Do we really think that anybody who accepts the above "is-ought" argument as reasonable, will not do the same for the above "is-is" argument? And if there are a few people who do not find the "is-is" argument reasonable, do we really believe they will find the "is-ought" argument reasonable? And even if a person were to accept the "is-ought" argument and nevertheless reject the "is-is" argument, would not this amount to his saying that what is an acceptable reason for saying that we ought not to punish an insane man, is *not* an acceptable reason for saying we do not want, we do not want others to want, we will not and will try to persuade others not to, punish him? And would not this defeat the purpose of getting him to say that we ought not to punish an insane man, of getting him to accept the "is-ought" argument, of not dispensing with "ought" statements?

Furthermore, in having supposed all along that we could not break through the is-ought barrier, we may have failed to realize that this is much more than a mere supposition. It is a real

impasse. Not only is there disagreement about whether we *can* break through the is-ought barrier, and here I suspect that there are more who believe we cannot than there are who believe we can, but even among those who believe we can there is disagreement about which "is" statements are to be considered acceptable or even relevant reasons in support of "ought" statements. Even the few who believe that ethical and moral statements have a logic of their own, seem to end up with logics of their own for ethical and moral statements.

Oh well, but we have taken a relatively trivial example of a man having committed a few murders, an example which can hardly serve as a model case of important ethical and moral conflict. When you consider something like contemporary totalitarianism, involving persecution, concentration camps, secret police, executions, destruction of freedom, denial of life, liberty, the pursuit of happiness, and all the other things in life we hold to be of greatest value, then we can see more clearly the importance, indeed the necessity of "ethics" and "morality". What could be a better example of "is" believers than a society ruled by dictators, tyrants, men who have dispensed with "ethics" and "morality" who are not in the least concerned with what "ought" to be done, only concerned with doing what they want, with what *is*.

Look here. It is not really fair to suggest that Hitler did not believe and say that we "ought" to persecute Jews, that Stalin did not believe and say that we "ought" to destroy bourgeois democracy, etc., but let that pass. It is hardly necessary to point out that some of the foulest deeds in history, the present-day brutalities of totalitarianism, have been committed in the name of "ought" statements.

But to get to the real point. Do we think that in a society of "is" believers, people are less likely to want and fight for life, liberty, and the pursuit of happiness, that they will want and accept concentration camps, persecution, secret police, etc.? Do we honestly believe that a person does not want to be put in a concentration camp or be executed because he believes that he ought not to be? Do we really believe that the development of democracy, the desire for freedom, etc., is a result of a series of "ought" beliefs, or do we believe that it is connected with what we know and could know about the kind of animal man happens to be and the world he lives in, which we can describe in terms of "is supportable" statements? It is true, of course, that some people want to and try to persecute others, destroy freedom, etc., but it is also true that some people also believe we ought to do



these things, so it will not do to say that using "ought" statements is more likely to prevent these things from occurring than dispensing with them.

Look at it this way. We happen to believe in democracy, freedom, we are opposed to totalitarianism, etc. It does not matter here whether we say we ought to believe in or want democracy. How do we go about attaining this end? If we continue to use the "ought" language we will continue to be stuck with finding the right "ought" statements to begin with, or the "is-ought" barrier. Remember, the totalitarians will also be using the "ought" language for their own ends. In the case of finding the right "ought" statements to begin with, the totalitarians will proclaim their self-evident truths just as we will. In the case of the is-ought barrier, we will be as unable to break through as they will.

Now suppose "ought" statements are dispensed with, then what? We will no longer have to worry about and spend time finding the right "ought" statements to begin with or bridging the gap between what "is" and what "ought" to be. We will be able to concentrate on finding "is" and "is supportable" statements, finding out why people want to persecute and enslave others, discovering the economic, social, political, psychological, etc., reasons for the existence of democracy, totalitarianism, etc.

We will be able to find out ways of promoting the one and preventing the other. Isn't this the sort of thing Dewey had in mind in reiterating the need for scientific method in ethics, in the social sciences?

Hold on. Saying we will be able to do these things, promote democracy, etc., does not mean we will *do* these things or even want to do them. And it does not mean that the totalitarians will not be able to or not actually try to promote totalitarianism. Also, all this talk about obtaining more knowledge, etc., and using it to promote democracy and prevent totalitarianism, how do we know that people will do this sort of thing or even desire to do this?

Oh well. Of course there is no guarantee they will do this, any more than there is that a world of "ought" believers will. But without "ought" statements or the gap between "is" and "ought" statements to bridge and the hindering results therefrom, the only thing they will have left to fall back on will be the search for "is supportable" statements, the acquisition of more knowledge, and so on. People will not spend their time telling each other they ought to support or destroy democracy, they ought to fight or adopt totalitarianism, etc. They will not spend their

time giving "is" reasons why they "ought" or "ought not" to do such and such, and finding themselves unable to show how from the "is" reasons they offer it follows that they "ought" or "ought not" to do such and such.

Yes, but what is wrong with doing both? Why can't we continue using "ought" statements, as well as "is" statements, thereby getting the benefit of both, just as we have been doing all along?

But the whole point has been to consider not merely whether we can do just as well without "ought" statements, but whether we can do even better without the long history of trying to break through the insufferable "is-ought" barrier. The point is that if we think we have to rely on "ought" statements, if we find it impossible to get from an "is" to an "ought", then we will have the kind of situation we do in fact have in the present impasse in ethics and morality. It is not a question of getting the benefit of both but rather getting the benefit of one without the hindrance of the other.

Well, this whole thing seems very odd, does it not? I mean, this attempt to show that we ought to do away with "ought" statements because of the is-ought barrier, and trying to show this by using "is" statements.

Not at all, there is no need to think we are trying to show that we "ought" to do away with ought statements. Rather we are merely wondering about what would happen if we dispensed with "ought" statements as compared to what we can expect if we do not.

But there seems to be another paradox here. After all, have not we presupposed all along that we ought to believe "is-supportable" statements only? And we cannot say that it is self-evident that we ought to do this, or that we have "is" reasons for doing this, can we?

True, but this is the same thing, for we have been merely considering what is likely to happen if we rely on "is-supportable" statements only, as compared to what we can expect if we do not.

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## VI.—MATERIAL IMPLICATION RE-EXAMINED

BY THEODORE C DENISE

GIVEN a sequence of valid arguments from ordinary discourse of the form  $p \cdot q$  and  $q, r \cdot s$ , the  $p$  and  $q$  statements consistent and the  $q, r$  and  $s$  statements consistent, does it follow that the argument of form  $p, r \cdot s$  is valid and that the  $p, r$  and  $s$  statements are consistent? If innocent of the logic of material implication, we would not, I think, hesitate to say it does, if guilty, we say only that the argument is valid and that its component statements may or may not form a consistent set. This is one way of indicating the restriction upon the range of consistency as compared to the range of validity which almost universally surprises those turning to the material logic for instruction in analysing ordinary argument. For some, a logic so "paradoxical" can be employed only with misgivings.

We accept the possibility of inconsistency in the third of the foregoing arguments as material logicians because, by uniform statement-substitutions for statement-variables in the three argument-forms, we can show such a set of arguments as

$P \cdot \sim P \supset Q, \sim P \supset Q, \sim P \therefore Q$  and  $P, \sim P \therefore Q$ ,

or again,

$P \cdot \sim Q \supset P, \sim Q \supset P, \sim P \therefore Q$  and  $P, \sim P \therefore Q$ .

Is the wider claim for consistency at issue here, the "natural" claim, defensible against this? I believe it is and will so argue. But is it defensible without demanding that the system of material implication be radically altered or else wholly consigned once and for all to the mathematicians? Again, I believe it is.

The resolution of the problem as stated depends on taking certain distinctions seriously, chiefly the distinction between a statement as asserted and a statement as postulated. The ramifications of this resolution extend quite obviously throughout the length and breadth of organized knowledge, or so it seems to me. But I shall not enter into the heady occupation of chasing ramifications at this time.

1. When asked for examples from language of material conditionals which are just that and not, perhaps, obscure causal or analytical conditionals or conditionals of some other sort, we turn typically to such statements as "If I have not taken leave of my faculties, this is Monday" ( $\sim F \supset M$ ) and "If this book is not subversive, then two plus two does not equal four"

( $\sim S \supset \sim E$ ) It is, however, commonplace to observe about such examples that they amount to assertions of simple statements, the first telling what day of the week it is and the second condemning a book. Taking this lead seriously, let us theorize about the way in which as listeners we come to regard  $M$  and  $S$  asserted.

If the day of the week is such that  $\sim M$  is true in the first instance or if the character of the book is such that  $\sim S$  is true in the second instance, we are prepared to charge our informant with error; accordingly, it is appropriate to say that the contradictory of each is asserted, that the unuttered simple statements  $M$  and  $S$  are *assertions*. It is only with an extra effort, perhaps the effort of humour, that we may speak of the conditionals  $\sim F \supset M$  and  $\sim S \supset \sim E$  as asserted in this sense. Neither an appraisal of the mental health of the speaker nor a review of arithmetic seems intended. Having no concern about  $\sim F$  as empirical or  $E$  as *a priori*, we have no concern about the possibility that the conditionals  $\sim F \supset M$  and  $\sim S \supset \sim E$  are erroneous, no concern about the possibility that the conjunctions  $\sim F \sim M$  and  $\sim SE$  are true.

$\sim F \supset M$  and  $\sim S \supset \sim E$  are *proxy-assertions* for the assertion of  $M$  and  $S$  respectively, i.e. each is stated *in lieu of* an assertion, but withal as an indication that a specific and determinable assertion is being made. Ordinary discourse abounds with proxy-assertions, I should say, and there is no single way in which they reveal the assertions for which they stand. In the present instance we may account for the assertion  $M$  from its proxy-conditional  $\sim F \supset M$  somewhat as follows.

We speculate about the antecedent of  $\sim F \supset M$  that it is topically irrelevant not only to the consequent but also to the conversation at large. This leads us to speculate that  $\sim F$  is not being used informatively, and thus, to reason from a sense of logical appropriateness that the connected  $M$  occurs non-informatively as well. It is at the same time clear that of the two components  $\sim F$  and  $M$  it is the latter which needs finally to be regarded informatively unless  $\sim F \supset M$  is wholly irrelevant to the topic of conversation. Confident that there is overall intent to inform, i.e. that either  $M$  or  $\sim M$  is actually being asserted, we nevertheless turn to the immediate alternative of regarding  $\sim F \supset M$  solely as a *postulate*, i.e. as a statement we are to regard as assigned the truth-value 'T' for deductive purposes.

$\sim F \supset M, \sim F \cdot M$  suggests itself at once as the required deduction, for the twofold reason that we are more in the habit of postulating  $\sim F$  gratuitously in a context where no special

cues obtain than any of  $F$ ,  $M$ , or  $\sim M$  and that we seek a conclusion free from both the topically irrelevant  $\sim F$  and  $F$ . Non-informative  $M$  derived from the postulated conditional and the postulate we provide (presumably as expected), we ourselves initiate the assertion of  $M$  in the name of the speaker.

The conditional of the second example, non-asserted but postulated  $\sim S \supset \sim E$ , leads to the deduction  $\sim S \supset \sim E, E \cdot S$  and so, finally, to the assertion of  $S$  for somewhat related reasons. It is so habitual to us to postulate  $E$  in opposition to  $\sim E$  that, no matter how inclined we are to believe either  $S$  or  $\sim S$  we move to postulate  $E$ . The psychological status of a familiar analytic statement is at play here and not directly its logical status. Any simple, not-to-be-contested-when-not-relevant "truth" might effectively replace the  $\sim F$  and  $E$  of our examples.

2. In one way, reflecting about material conditionals in language is pointless. They occur infrequently and then, perhaps, not always as proxy-assertions. The importance of  $p \supset q$  to us derives from our practice of placing conditionals of language which we do not think are of this form into this form. If there were no material conditionals in language, our practice would not change. We use this form because it proves a systematic way to assure the validity of all valid arguments involving conditionals of whatever kind. But in another way, such reflection is instructive, it is instructive because, as I think, it focuses attention on neglected but nevertheless crucial characteristics of ordinary argument generally.

We distinguish between valid arguments and sound ones, the former class including the latter but not conversely. There is an established sense in which we speak of valid arguments whose premisses may in fact be erroneous. Such expressions as "If the premisses were true then the conclusion would follow validly" do not mitigate this distinction, since each pairs off with a countering expression, as, in this instance, "Even if the premisses were true the conclusion would not follow validly". In so far as we deal deductively with the premisses of ordinary arguments, we do so by regarding them solely as postulates. Clearly logical demonstration involves us in doing precisely this; if it did not, we could never decide with certainty about the validity of any argument containing a contingent statement.

What we normally call a *premiss* in ordinary discourse is distinguishably taken both as an assertion and as a postulate in the senses indicated (I am placing analytic statements and tautologies beyond the bounds of the present discussion). In so far as premisses are taken as informative, they are assertions,

and, in so far as they are taken as sources for deduction, they are postulates. Following through, statements logically derived from premisses can in all strictness be no more than proxy-assertions for their own assertion since they depend directly on premisses taken as postulates and only indirectly on premisses taken as assertions.

Insisting on the distinction between a statement as asserted and as postulated, let us examine the following representative linguistic situation. Suppose in the course of a conversation I remark that a mutual friend, John, is presently in South America ( $\vdash J$ , the familiar assertion sign adjusted to our purpose), and then, at a later juncture, I say "If John is in South America, his wife, Kate, is unhappy" (ignoring that the conditional may be interpreted causally,  $\vdash J \supset K$ ). Is it not clear that I authorize the assertion of  $K$  whether wittingly or unwittingly? This example differs from the examples of the earlier section in that the conditional is asserted directly as well as postulated. It is further to be observed that my having already made explicit my belief in the truth of the antecedent produces an effect comparable to the effect produced before by topical irrelevance and our habit of postulating some things more readily than others.

The latter assertion,  $J$ , joins with the earlier assertion,  $J \supset K$ , to form a set of premisses from which  $K$  is first derived, the premisses being postulates, and then asserted, the premisses being assertions. (Let us symbolize the argument conveniently as  $\vdash (J \supset K, J) \cdot \vdash K$  where—the absence of an assertion sign indicating a postulate or a derivative—the following is intended. Since  $\vdash (J \supset K, J)$  and  $J \supset K, J \cdot K, \vdash K$ .) The question why the listener concerns himself with drawing this inference or any inference remains speculative, but the question of the justification of the inference is settled.

It is a crucial feature of a unit of discourse that earlier assertions are not sealed off from later assertions unless so indicated, say, by a parenthetical direction like "forget about that other remark". To put the matter more officially, it is understood by speaker and listener alike that in a speaker's developing unit of discourse *a present assertion may be joined with any or all earlier assertions to form a set of premisses*, i. e. each assertion in a unit of discourse is *premiss-joining*.

As a check that I have not applied the predicate "premiss-joining" too generally, let us turn to another familiar linguistic situation. Suppose I argue successively "If Ivan is there Carol is, too, and since I know he is there that is where she is" ( $\vdash (I \supset C, I) \cdot \vdash C$ ), and "If Ivan is there, so is Doris; then, Doris

is there" ( $\vdash I \supset D \therefore \vdash D$ ). Surely my second argument is as acceptable as my first. Asked about its exact form, we simply bring the assertion,  $I$ , of the earlier argument forward and display  $\vdash (I \supset D, I) \therefore \vdash D$ .  $\vdash I \supset D$  is premiss-joining even though the premiss it joins is in an earlier, distinct argument.

Statements which are solely postulated do not show a characteristic comparable to premiss-joining. Altering the foregoing example with such introductory rubrics as "assume that", so that the two arguments become properly  $I \supset C, I \therefore C$  and  $I \supset D \therefore D$ , we are left simply with two explicit arguments, one valid and one invalid. The fact that we immediately see what could be assumed to make the second one valid, does not in itself provide us with the right to assume it. If the postulate  $I$  is to be carried over into the second argument, we must be so instructed.

A final observation about the premiss-joining characteristic of assertions will lead us back to the problem set forth at the beginning of this paper. If in the course of a natural conversation someone actually does argue in a way suggesting one of the various "paradoxical" forms of material implication, how are we to analyse it? Let the argument be "Today is Tuesday; so either I did not bring my book or this is Tuesday". With respect solely to deduction there seems to be no case for anything other than  $T \therefore \sim B \vee T$ . But with respect to the statements of the argument regarded not only as postulates or derivatives but also as assertions or proxy-assertions, I submit  $\vdash T \therefore \vdash (\sim B \vee T, T)$  captures our understanding of, and the presumed intent of, the argument, whereas  $\vdash T \therefore \vdash \sim B \vee T$  does not. The point to note is that in such an instance as this a derivative is a proxy-assertion for itself only when it is to be joined with the premiss from which it derives. No rule of this sort seems to be in play for "non-paradoxical" forms of argument, e.g.  $\vdash (A \supset B, A) \therefore \vdash B$  constitutes an acceptable symbolization of an argument.

I believe this rule articulates in part what objectors to the logic of material implication are concerned to say. The rule can only be properly developed—properly liberalized—in a context more technical than is here desirable, but it is instructive to apply it in its present form to the validity-consistency problem to show both how much and how little it advances us.

Substituting uniformly for statement-variables in  $p \therefore q$  and  $q, r \therefore s$  and  $p, r \therefore s$  so as to obtain  $P \therefore \sim P \supset Q$  and  $\sim P \supset Q, \sim P \therefore Q$  and  $P, \sim P \therefore Q$  yields a sequence of arguments requiring the interpretation that each statement is in each instance either solely a postulate or solely a derivative. The following considerations show this to be so. (1) When  $P$  is considered as a

premiss in the first argument, our newly mentioned rule requires  $\vdash P \therefore \vdash (\sim P \supset Q, P)$  (2) When what is asserted as a consequence of the first argument is again asserted as a premiss of the second, the second argument becomes  $\vdash [(\sim P \supset Q, P), \sim P] \therefore \vdash Q$  (3) So written, the second argument has inconsistent premisses rather than consistent premisses as specified. As far as language is used assertively, the kind of demonstration we offered to show the restriction on the range of consistency as compared to the range of validity is without relevance

Extending the assertion-postulate distinction to statement-forms, we may say that the rules of statement-substitution for the sequence  $\vdash p \therefore \vdash q$  and  $\vdash (q, r) \therefore \vdash s$  and  $\vdash (p, r) \therefore \vdash s$  are other than those for the sequence  $p \therefore q$  and  $q, r \therefore s$  and  $p, r \therefore s$ . The assertive sequence and its rules capture what is usual in discourse and the assumptive sequence and its rules capture what is usual in formal demonstrations. A crucial ambiguity having been located, the problem initially proposed stands resolved. But it may fairly be asked if in winning the battle the war has not been lost. The second argument under the assertive interpretation is now seen to be  $\vdash [(\sim P \supset Q, P), \sim P] \therefore \vdash Q$ . It is clearly inconsistent, if it is therefore not valid, why not?

3. We naturally respond in either or both of two ways upon hearing a contradiction. The first of these ways is represented by "If I grant this, then I must grant everything", and the second by "You end in telling me nothing". The first of these responses suggests that we regard the contradictories as postulates, and the second suggests that we reject them as assertions. Put bluntly, the interpretation I think called for is this: *Contradictory statements can only occur in an individual's unit of discourse as postulates.*

In postulating  $P$ , I assign it, shall we say, the truth-value 'T'. This is a positive and single act, i.e. in terms of the pair of statements  $P$  and  $\sim P$ , I assign the former its value without direct regard for the latter. Using a circle to represent postulation, we may represent the circumstance of postulating  $P$  as  $(\textcircled{P})$ ,  $\sim P$ . I do not automatically assign  $\sim P$  'F' in the doing, it is only in the absence of postulating  $\sim P$  additionally in the immediate context of deduction that  $\sim P$  is understood to take on the opposed truth-value. Thus the argument  $P, \sim P \therefore Q$  remains admissible, i.e.  $(\textcircled{P}), \sim (\textcircled{P})$  is not beyond the possibilities of postulation.

In contrast to postulating  $P$ , I engage in a double act when I assert it. I not only claim that  $P$  is true but also that  $\sim P$  is



false, i.e. I identify the condition of my own error even as I identify the condition of my own correctness. Using a stroke to indicate the statement claimed erroneous and the absence of a stroke to indicate the statement claimed correct, we may show  $\vdash P$  by  $(P, \sim \bar{P})$  and  $\vdash \sim P$  by  $(\bar{P}, \sim P)$ . The seeming circumstance of  $\vdash (P, \sim P)$  would then be  $(\bar{P}, \sim \bar{P})$ , but this would make the claim of correctness and error simultaneously for both members of the pair. accordingly, no claim for correctness as opposed to error would be made for either, i.e. neither  $P$  nor  $\sim P$  would stand asserted.

Allowing this approach to the matter, we may say of any assertion in an individual's unit of discourse not only that it is premiss-joining, but also that it is *consistent with every other member assertion*. Or, speaking more exactly, we may say: An assertion is such that no two assertions in an individual's unit of discourse make different claims for correctness and error with respect to the same pair of statements.

We are now prepared to deal with the  $\vdash [(\sim P \supset Q, P), \sim P] \therefore \vdash Q$  with which we were left at the close of the preceding section. This is seen to be illegitimate since we cannot assert both  $P$  and  $\sim P$ ; furthermore, corrected to  $\vdash (\sim P \supset Q) \therefore \vdash Q$  it is neither valid nor related sequentially to the first assertive argument as it has been shown to be, viz.  $\vdash P \therefore \vdash [(\sim P \supset Q), P]$ .

It is then by means of the distinctions reviewed that the sting is removed from the "paradoxes" of material implication and that careful correction may be made in the use of the logic of material implication and formal systems generally.

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## VII.—DISCUSSIONS

### ON CARNAP'S VERSION OF LAPLACE'S RULE OF SUCCESSION

1 In this note I shall confine myself to some of the most elementary models of probability theory. My model consists of a set of boxes, each containing the same number,  $n$ , of buttons. Each of the buttons has either the property  $A$ , that is, its colour is *amber*, or the property *non-A*, that is, its colour is *not amber*. I shall assume that this is black, and I shall use ' $B$ ' ('*black*') for *non-A*.

We assume that in every box of our set are  $n$  buttons. We further select *one* of the boxes, it does not matter which. We assume that we *know* that the number of buttons in a sample taken from this box is  $s$ , that the number of amber buttons in the sample is  $s_A$ , and that the number of non-amber (that is, black) buttons in the sample is  $s_B$ , so that

$$s = s_A + s_B \leq n.$$

The one selected box is to represent our empirical universe. (The other boxes are introduced merely in order to determine the probabilities of  $A$  and  $B$  in the selected box.)

2 I shall confine myself here throughout to an extremely simple case—to the case in which we *know* that

$$s_B = 0, \quad s_A = s = n - 1, \quad (1)$$

that is to say, I shall assume that *all but one* of the  $n$  buttons in the box are *known* to have property  $A$ , or amber. And I shall confine myself to the extremely simple problem:

(Pr<sub>1</sub>) Given the information (1), what is the probability  $P$  that the one unknown button in the box is, like all the others, also amber?

This problem is the simplest case of what Carnap calls the problem of '*the singular predictive inference*', and of which he says. 'It is of basic importance for inductive logic.'<sup>1</sup>

A more dramatic formulation of the same problem—but equivalent in at least one interpretation—is this.

(Pr<sub>2</sub>) We take the box, open it with closed eyes, remove one button and *cover* it (by placing a handkerchief over it). Then we

<sup>1</sup> See R. Carnap, *The Continuum of Inductive Methods*, 1952 (here briefly referred to as '*Continuum*'), p. 13. It seems indeed that this problem, and his proposed solution, is of decisive importance for Carnap's inductive logic, but for this very reason I wish to make it clear that my present criticism of his solution, though it may incidentally hit Carnap's theory of induction, is not intended to be read as a criticism of this theory. For I have criticised the probabilistic theory of induction—not only that of Carnap, but also that of Keynes, Jeffreys, and others—in a far more general way in my *Logic of Scientific Discovery*, especially on pp. 390 f.

open our eyes and see that all the remaining buttons in the box, without exception, are of amber colour. What, then, is the probability  $P$  that the button first removed and so far unobserved is also amber, and that, therefore, all buttons in the box, without exception were of the property  $A$ ?

3. The solution of our problem is made very easy by the following two assumptions A1 and A2 (of which A1 will be found to be a consequence of A2)

A1 The total (finite) set of boxes of size  $n$  ( $n$  is also finite) from which our box was chosen contains an equal number of amber and of black buttons. We can express this by

$$p(A) = p(B),$$

in words, the (absolute) probability of  $A$  equals that of  $B$

A2 The total (finite) set of boxes of size  $n$  from which our box was chosen contains an equal number of boxes with 1 amber button, with 2 amber buttons; with 3 amber buttons; . . . . Thus the probability of selecting a box with 1 amber button or with  $n - 1$  amber buttons will equal that of selecting a box with only amber buttons (that is, with  $n$  amber buttons).

Assumption A2 postulates that the set of our boxes has a 'Laplacean distribution' (or 'rectangular distribution'). It leads to the probability distribution of Carnap's preferred 'confirmation function',  $c^*$ .<sup>1</sup>

We now introduce the following names of statements.

' $a$ ' is the name of the statement 'All buttons in our selected box are amber' (or ' $(x)Ax$ ')

' $b$ ' is the name of the statement 'Precisely one button in our box is black' (or ' $(\exists x)(y)(Bx \supset (By \supset x = y))$ ').

It is clear from assumption A2 that

$$p(a) = p(b). \quad (2)$$

Writing ' $p(x, y)$ ' for 'the (relative) probability of  $x$ , given the information  $y$ ', we can now re-formulate our problem (Pr<sub>1</sub>) as follows (Pr<sub>3</sub>) What is the numerical value of the probability

$$P = p(a, a \vee b) ?$$

The  $p$ -symbol here means 'The probability of the statement  $a$ , given the information  $a$  or  $b$ '; and this, again, means, 'The probability of all buttons being amber, given that either all are amber or exactly one is black.'

<sup>1</sup> See R. Carnap, *Logical Foundations of Probability*, 1950 (here briefly '*Foundations*'), § 110, A, where the functions  $c^*$  and  $m^*$  are explained; also *Continuum*, p. 45. Our A2 is used by Carnap in order to define his  $c^*$  and  $m^*$  functions, he expresses it by saying that 'all structure descriptions have equal  $m^*$  values'.

The solution of this problem is very simple. Since  $a$  and  $b$  are incompatible, their conjunction,  $ab$ , has zero probability

$$p(ab) = 0. \quad (3)$$

From this follows that

$$p(a, a \vee b) = p(a) / (p(a) + p(b)), \quad (4)$$

and therefore, by (2),

$$P = p(a, a \vee b) = 1/2 \quad (5)$$

This is the solution to our problem

The same result (5) can be derived in a Carnapean system of  $n$  individuals, using his  $c^*$ -function. we obtain (2) because  $a$  and  $b$  are what he calls 'structure-descriptions': they are equally probable if  $c^*$  is adopted. And since (3) also holds for structure descriptions, we obtain (5). However, Carnap's own solution—that is, our formula (6), stated below—is incompatible with our result (5).

4 Two things about this result may be worth commenting on.

(a) Our formula (5) does not refer to  $n$ . It is, therefore, valid for boxes of all sizes (and it may even be extended to infinite samples). It shows that, even on the assumption that we have checked all buttons of a sequence (even an infinite sequence) except, say, the first, and found that they all have the property  $A$ , this information does not raise the probability that the as yet unknown first button has the property  $A$  rather than  $B$ .

Thus the probability of a universal law—such as the statement  $a$ —remains, on our assumptions  $A1$  and  $A2$ ,<sup>1</sup> equal to  $r = 1/2$  even if the number of supporting observations becomes infinite, provided one case of probability  $r = 1/2$  remains unobserved. (From this it is obvious that the probability of a genuinely universal law, given any observational support, will remain close to zero even in a very large finite universe, and must go towards zero with increasing size of the universe.)

(b) Formula (5) shows that for this simplest case of the 'singular predictive inference', Carnap's method of choosing his preferred 'confirmation function' agrees with those methods of Peirce's, Keynes's, and Wittgenstein's<sup>2</sup> which Carnap decided to discard because he could show that they are 'in striking contradiction to the basic principles of inductive reasoning'<sup>3</sup> since they make it

<sup>1</sup> It may be noted in passing that the result (5) also obtains if we replace  $A2$  by the assumption that (i) the probability distribution of the set is not Laplacean but Bernoullian, and that (ii) the position of the one button in the box (whether the first, say, or the last), whose colour is not known, is determined in advance. If only (i) is assumed but not (ii), then  $P = 1/n$  and thus for  $n > 2$ ,  $P = 1/n < 1/2$ . Thus in this case, the probability of the universal law diminishes with increasing  $n$ , and goes to zero, even if we are given the information that an infinite number of amber buttons, and only amber buttons, have been observed.

<sup>2</sup> See *Continuum*, p. 40, *Foundations*, § 110. A p. 565

<sup>3</sup> See especially *Foundations*, loc cit

impossible to *learn*, inductively (by 'simple enumeration'), from experience. (I have criticised this kind of 'transcendental' argument in my *Logic of Scientific Discovery*<sup>1</sup>)

5. Both Laplace and Carnap obtain a very different result for our case,  $s_A = s = n - 1$ , they both obtain the solution

$$P = (s_A + 1) / (s + 2) = n / (n + 1). \quad (6)$$

This equals  $1/2$  only for  $n = 1$ . Thus for  $n = 99$ , say, the solution according to Laplace and Carnap<sup>2</sup> is  $P = 0.99$ , rather than  $1/2$ .

Now Laplace's argument seems to me capable of a correct interpretation (because he can express order); but Carnap's is mistaken. In fact, our solution (5) follows immediately from Carnap's system; and since he obtains (6), his system must be inconsistent (at least on the assumption that there exists a box with more than one button in it).

6. It is not necessary to defend (5). It follows from (2) which is an immediate consequence of Carnap's assumption (which is our A2) that all 'statistical distributions' or 'structure descriptions' have equal probability; for  $a$  and  $b$  are each a 'structure description' in Carnap's sense. What may be necessary is to give an explanation of his mistaken result (6).

7. In order to prepare for this explanation I shall first give a reconstruction of Laplace's argument. I shall interpret it in such a way as to leave no doubt that it is valid.

We can put this argument by referring to our problem (Pr<sub>2</sub>). There we selected first a button and covered it up. This introduces another property in addition to  $A$  and  $B$ —the property of being covered (or, say, of being *unknown*). Let us call this property ' $C$ ', and let us assume, as in our example, that  $C$  means 'being the one and only button in the box which has been covered'. (Thus  $C$  is so defined that from ' $Cx$ ' and ' $Cy$ ',  $x = y$  can be derived.) It is clear that, on the information that precisely one button has been covered at random, the probability will be, for each button,

$$p(C) = 1/n.$$

Now let us denote by ' $c$ ' the statement 'The one button covered up is black'. (Or, ' $(\exists x)(Bx \cdot Cx)$ '.) Then the probability of  $bc$ , that is, of the conjunction of  $b$  and  $c$ , will be.

$$p(bc) = p(b) \cdot p(C) = p(b)/n \quad (7)$$

and thus, by (2),

$$p(a, a \vee bc) = p(a) / (p(a) + p(a)/n) = n / (n + 1). \quad (8)$$

<sup>1</sup> See my *Logic of Scientific Discovery*, 1959, 1960, appendix \*vi, pp 368, 370

<sup>2</sup> See *Continuum*, pp 35 (12-7), and 45 (15-1), where  $k$  equals the number of the properties, in our case 2 ( $A$  and  $B$ ). Carnap criticises Laplace on p 35, but his criticism depends on interpreting Laplace so as to have ' $M$ ' instead of ' $c$ ' in (12-7), and on choosing  $k \neq 2$

This result is correct; but it depends, essentially, upon the introduction of a *third property* which, like our  $C$ , can hold only for *one* of the buttons in the box.

In Laplace's case, this property  $C$  is the *position* of the button in a sequence in which the buttons of the box are ordered: it may be the first (as in our  $(Pr_2)$ ) or the second or the last button in the sequence in which the buttons are drawn out of the box: or, for example, the *order in which the buttons were observed* may be used, and  $C$  may be 'the *last* button (and therefore, at a certain moment, *the only one not yet observed*)'. Or else,  $C$  may be explained, simply, as the property 'the one and only unobserved button'.

In the presence of such a  $C$ , the result (8) is not only correct but intuitively clear: were our box to contain not  $n$  but only  $n - 1$  *amber* buttons, then it would be a strange and improbable coincidence that this one button was the last one to be observed. This is what makes  $p(bc) = p(b)/n$  small, as compared with  $p(b)$ ; and it is precisely this smallness of  $p(bc)$  which produces the difference between our (5) and Laplace's (8).

8. In Carnap's case, however, no such third property  $C$  is available apart from  $A$  and  $B$ , in fact, order and positional properties are not expressible in Carnap's system, and the *temporal* predicate 'not yet observed' or 'last observed' is inexpressible in his language systems  $L$ . Moreover, we may lay down, in his system, that the language under consideration contains *only* the two predicates,  $A$  and  $B$ , and we may yet obtain on this assumption from Carnap's formulae<sup>1</sup> the result (6), which contradicts our (5) which is also derivable in Carnap's system.

How then does the contradiction arise, and how can it have remained unnoticed so long? First, because (6) is intuitively far from absurd, since we may *unconsciously* operate with a predicate  $C = \text{unobserved}$ , as explained above. Secondly, because Carnap operates with the idea of individual buttons each of which has a *name* in his language. But this is a very dangerous assumption which, in probability theory, may lead to contradictions, as I have shown here; for it may tempt us to use these names as if they were predicates, or as if they were indicating position, or order. If we assume, more particularly, that we know the name (or ordinal number) of *the next button to be observed* then we have succumbed to this temptation.

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<sup>1</sup> See *Continuum*, p. 45 (15-1).

## PROOF-MAKING

It is unfortunate but inevitable that the techniques and concepts employed in the actual development of logic and mathematics presuppose certain answers to philosophical problems which could be given different answers. Consider, for example, how difficult it is to think of mathematical laws as being more like rules and imperatives than statements when the whole development of formalized mathematics is fashioned in terms of the logic of statements. This particular difficulty is not one which I wish to pursue here, but it was because of difficulties similar to, though less far-reaching than this, that I tried to look at the problem of deductive completeness through the eyes of an imaginary Euclid. That is, from the bottom up rather than from the top down. Mr. Parsons and Mr. Kohl, on the other hand, in their criticism of my heresies,<sup>1</sup> look the other way, and of course they get different answers. Perhaps, however, the differences between us are not so great as they appear to make them, and I should like to see if I can narrow the gap by looking at the problem from their end.

We have, let us suppose, a vocabulary which consists of a set of symbols of various kinds, and we define a formula as any concatenation of the symbols of the vocabulary. Next we lay down a set of formation rules, which are expressed entirely in terms of the syntactical patterns of the symbols, and which serve to separate well-formed formulae from the rest. From among these we next select a certain set and call them axioms. Finally we lay down a set of transformation rules which are again expressed in terms of the syntactical patterns of the symbols and which enable us to separate off a special class of the wff which we call theorems. We now have a formal system *S*. It is characterized by its vocabulary, formation rules, axioms and transformation rules, and other formal systems are distinguishable from it according as they differ in one or more of these particulars. No interpretation whatever is put upon the symbols at this stage. We simply have the symbols, rules for juxtaposing them, and rules for manipulating them. Hence it is inappropriate to use the descriptions 'true' and 'false' of any concatenation of the symbols, theorem or not. It is exactly like saying which bits of wood are to count as chess pieces, which not, how the board is to be set up, and what moves may be made.

Next we lay down a further set of rules, rules of interpretation. Commonly these rules will specify sets of values for the various kinds of variables, connective words and phrases for the logical constants, and designating words and phrases for the designating constants. Thus we have now stitched a semantic theory to our syntactical game and we have what we may call a formal language. The wff are now sentences, and they express statements about a set of "objects" (numbers, *e.g.*) and the relations between them. Some of these

<sup>1</sup> "Self-reference, Truth, and Provability", *MIND*, n s lxx (January 1960)

statements will be true, some false. And if, in particular, the axioms express true statements, then we may say that the set of objects and relations between them constitute a model of the formal system.

Theoretically it is possible to set up a formal system *ad hoc* and then to discover a model for it. Usually, however, a formal system will have been developed as an interpreted system, the axioms will have been taken as true statements about the objects of a model and proofs will have been arguments which establish the truth of other statements about those objects. That is to say, the formal system will have been developed as a formal language, and there will have been no distinction between what is provable and what is true. Only at a later stage in the development will the formal system have been abstracted from the formal language and a distinction made between truth and provability.

Once the distinction is made, however, three questions arise. The first is whether any other interpretation will satisfy the axioms. That is to say, does the formal system have another, or several more models? Sometimes the answer to this is yes. In such a case we may say that we have a system of formal languages with a common syntax, the languages differing in their semantics. If the formal system is  $S$ , I shall denote the different formal languages by  $S_1, S_2$ , etc. Thus  $S_1, S_2$  are different interpreted versions of the uninterpreted formal system  $S$  such that the axioms go over into true statements. One would expect, therefore, that the criteria of truth with respect to each of the formal languages will be different. And I take it that this is what Parsons and Kohl mean when they say "the notion of truth is relative to an interpretation of the system", contrasting this with the notion of provability which is relative to the system as a whole, not to the interpretations of it.

The second question which arises is whether all the provable statements of  $S_n$ , for a given  $n$ , are true. This is the question of plausibility. It is not a question which arises if one begins with an interpreted system and argues validly from true premisses to conclusions. For what we mean by a valid argument is just that if the premisses are true then the conclusion is true. Hence if we suppose that the axioms (premisses) are true then the theorems (conclusions) are true. Perhaps, however, it might be taken as the question: are the arguments valid?

The third question which arises is the completeness question: are all the true statements of  $S_n$ , for a given  $n$ , provable?

Now consider how and why the last two problems arise in an historical way. We begin with an interpreted system  $S_1$ , say, and we argue from statements which we assume to be true to statements which we conclude are true, and then from these conclusions to others. This systematic development of arguments we call proof-making. We then make a further and different development, a methodological development. We abstract from the language  $S_1$  to the formal system  $S$ , and we find that the proof-making technique is



wholly definable as a characteristic of  $S$ . Moreover, since  $S$  contains no statements, but only uninterpreted formulae,  $\tau \in$  expressions which have a sentential structure but no content, we see that the technique of proof-making is independent of the meaning of the statements of  $S_1$ . It is a formal enterprise, entirely syntactical in character. So we describe the end-products of the application of this technique as provable expressions. When, however, we recover the language  $S_1$  by laying down the rules of interpretation as a separate item, the formulae of  $S$  take on a content as well as a form. And taking the content, or meaning, into account, we may ask if they are true or false. Thus the two concepts, truth and provability, in terms of which the problems of plausibility and deductive completeness are expressed, arise from the two characteristics of any expression: its content and its form.

It now begins to look as if the provability and the truth of a given expression can be established as separate items of information. It looks as if, having produced all the theorems in the formal theory, we may then specify what the interpretation is to be, take the content of the theorems into account, see what they mean, take a look at the facts (the objects and relations between them which constitute the model), and so decide if the interpreted theorems are true.

I suspect that it is the sort of picture which Parsons and Kohl have in mind when they say, for example, that

"For every natural number  $\tau$ , there is a natural number  $y$ , such that neither is it the case that the sum of  $\tau$  and  $y$  is equal to the sum of  $y$  and the successor of  $\tau$ , nor is it the case that the product of zero and one is equal to one,

is obviously true", and that the statement which Gödel's formula expresses is true in "the same sense" ( $\tau \in$  obviously?)

But there is something very wrong with this picture. For when the facts are relations between numbers, or when the fact is the unprovability of a formula of  $S$ , there is no way of inspecting them. It is not at all like looking at birds and trees and seeing that branches make good perches. And it is just this sort of implied carry-over from the techniques of checking every-day statements to those for checking mathematical ones that I wanted to guard against in my earlier paper. If it were like this, if we had to rely on inspecting the objects of a model in order to check the truth of mathematical theorems, or even if we could check in this way, then proof-making would be idle. Why bother with proofs if we can establish the truth of a theorem independently of the argument which supports it?

Perhaps Parsons and Kohl are not thinking in this way, and I do not want to put words into their mouths. But it is a picture which fascinates some. And even if it is acknowledged that numbers and the relations between them are not "out there" to be inspected, as birds and branches are, still the independent check is rarely forfeited. Perhaps, for example, we intuit the relations between the numbers. However it is, somehow we know, independently of our

arguments, that such and such is so. And this second criterion of acceptance, however it is characterized, which is employed in order to check the carefully contrived and elaborate technique which we already have—proof-making, is itself not open to check. It is ultimate.

There is a whole philosophy built into this way of looking at mathematics, and the conventional development fosters it. For this reason it is misleading of Parsons and Kohl to say that their discussion is not a "philosophical explication of mathematical truth" but, rather, a description of the "formal properties of the notions in question, which any philosophical explication must explain rather than deny" (p 73). In fact, this description is a philosophical account of the concept of mathematical truth, namely that conventional account in terms of which the "formal properties of the notions in question" are formulated. For these formal properties are not facts to be explained, but concepts which are already part of an explanation.

Yet if there is something wrong with this picture how, it will be asked, do we establish the truth of a mathematical statement? By proving it; there is no other way. For a proof is simply a set of reasons which establish the truth of the conclusion. But if we grant this, we can only know that the theorem is true if it is the conclusion of a valid argument with true premisses, and even supposing that there are no qualms about the validity, we yet have to establish that the premisses are true. So, pushing back and back, we eventually arrive at statements which cannot be proved. These are the axioms, and their truth has to be established in some other way.

This is the heart of the matter and the heart of the mistake. For it is not that we check the axioms against the objects of the model, as we might check statements about cows by looking in fields, but rather, by supposing them to be true (i.e. by interpreting them in a given way) we define a set of "objects". That is, we create the model by making the assumption, not the truth of the assumption by taking the model. The interpreted formal system and the objects of the model are not two separate and independent entities, one a map of the other, instead, the interpreted formal system defines a set of theoretical objects which do not have a separate existence. There are cows and horses and we can count them. But cows and horses and counting are not the subject matter of arithmetic. Numbers, and the relations between them, are; and numbers are not to be found lying about in fields, real or intuited. Of course, we use these theoretical concepts, numbers, when we count cows and horses, but this is an additional step, a step which consists in applying the model, and the counting itself is not in any way a verification either of the statements of the formal language or of the statemental matrices of the formal system.

I do not want to conclude from any of this that because the Gödelian expression is not provable in  $S$  so it is not true, or

alternatively, that it is not known whether it is true or false. What I want to say only, is that to say that it is true is just to say that it is provable in *some* formal system; and to say that it is true over some model of *S*, though not provable in *S*, is just to say that the system in which it is provable, *S'* say, is different from, but bears a special relation to, *S*.

Parsons and Kohl object to what I say because they think that I have confused the kind of relativity associated with truth (relativity with respect to a model) with the kind of relativity associated with provability (relativity with respect to a formal system). But suppose for a moment that it is the case that the Godelian sentence *G* is not provable in *S* but is provable in *S'*. This implies that *G* is a true statement over the models of *S'*, not over the models of *S*. Hence, unless *S* has a model in common with *S'* (which is what I wanted to show in the later part of my paper, though perhaps I did not express it very clearly), there is no argument from the proved unprovability of *G* in *S*—which is equivalent to a proof of *G* in *S'*—to the truth of *G* over one of the models of *S* (i.e. to the truth of *G* in one of the languages *S*<sub>1</sub>, *S*<sub>2</sub>, etc.). We conclude only to the truth of *G* over the models of *S'* (of argument ζ of “‘True’ and ‘Provable’”). Here, therefore, the relativity associated with the concept of truth is not simply that which occurs as between the various interpretations of one formal system; it is instead the relativity which occurs as between two different formal systems. The complete story, therefore, is much more complicated than they acknowledge, and it is as follows.

We have a formal system *S* and we may suppose that *S*<sub>1</sub> is the standard, arithmetical interpretation of *S* and that *S*<sub>2</sub> is that (syntactical) interpretation in terms of which statements about the syntax of *S* can be made (*S*<sub>2</sub> is the language *A* of my earlier paper). The rules for the latter interpretation are implicit in the Godelian numbering technique. We now construct the following formula of *S*

$$\sim (Ey)P(y, sb(x, x)) \quad . \quad . \quad . \quad . \quad . \quad (1)^1$$

which can be interpreted in two ways—as an ordinary arithmetical statement, i.e. as a statement of *S*<sub>1</sub>, and also, as a statement of *S*<sub>2</sub>. In terms of this latter interpretation, the formula reads, ‘it is not the case that there is a number *y* which is the number associated with a formula, or set of formulae, which constitutes a proof of the formula with number *sb(x, x)*’, or, simply, ‘there is no proof of the formula with number *sb(x, x)*’.

The expression ‘*sb(x, y)*’ stands for a recursive function of natural numbers (i.e. its value can always be calculated in a systematic way) such that if ‘*a*’ and ‘*b*’ are numerals, and if ‘*b*’ is substituted for the free variable in the formula with number *a*, the value of *sb(a, b)*

<sup>1</sup> This is the formula (5) of Parsons and Kohl. I use a different symbolism for reasons which will become apparent presently.

is the number of the formula so obtained. Hence,  $sb(a,a)$  is the number of the formula which is obtained by substituting the numeral 'a' for the free variable in the formula with number  $a$ .

Now the formula (1) has a number associated with it. Let it be  $a$ . Form now the formula,

$$\sim (E y) P(y, sb(a,a)) . . . . . (2)$$

by substituting the numeral 'a' for the free variable in (1). The formula (2) so obtained has a new number different from  $a$ , and we calculate it as follows

(2) was obtained by substituting the numeral 'a' for the variable in the formula with number  $a$ . Hence it has the number  $sb(a,a)$ . Now (2), when interpreted as a statement of  $S_2$ , reads 'there is no proof of the formula with number  $sb(a,a)$ '; and we have shown that it itself has the number  $sb(a,a)$ . Hence it expresses the fact that it itself is unprovable. This is the Godel formula. Let the value of  $sb(a,a)$  be the number  $n$ . Then we may say that a natural-language equivalent of (2) is,

The sentence with number  $n$  is not provable,

where the sentence with number  $n$  is the one on the line above. By choosing  $n$  as the number of (1), which is not the Godelian formula anyway, and by obscuring the function 'sb' in an over-economical symbolism, Parsons and Kohl in fact obscure the issue.

So we have that (2), a formula of  $S$ , may be interpreted either as an arithmetical statement of  $S_1$  or as that syntactical statement of  $S_2$  which expresses the fact that it itself is not provable in  $S$ . Parsons and Kohl now assert, on the basis of the Godelian numbering technique, "that the statement of arithmetic which Godel's formula may be interpreted to express is true if and only if the formula is itself not provable in the system  $S$ " (p. 70). That is,

(2), when interpreted as a statement of  $S_1$ , is true

if, and only if, (2) is not provable in  $S$  (3)

and they seem to think that because we can demonstrate that (2) is not provable in  $S$  so, since we have this equivalence, we automatically establish the truth of (2) in  $S_1$ , and no other formal system comes into consideration, the whole thing has been worked entirely in terms of  $S$  and its various interpretations.

But (3) is not a statement of either  $S_1$  or  $S_2$ , since it relates them both. And to say that it is true is to say that it follows from the numbering technique. That it does, however, cannot be established in  $S$ , for we cannot, within  $S$ , talk about the arithmetization of the syntax of  $S$ , nor, for that matter, can we use the word 'true' or a symbol which can be interpreted as 'true'. Hence, it is not a statement of some other interpreted form of  $S$ . In fact, (3) can only be established in a language in which it is possible to talk about the numbering technique, that is, a language which embraces both  $S_1$  and  $S_2$ . Let this language be  $S'_2$  (the language  $L$  of my previous paper) which is

itself an interpreted version of a formal system  $S'$ . Suppose, further, that since  $S'_2$  is strong enough to establish (3), it is strong enough to establish,

that (2) is not provable in  $S$  (Gödel's theorem) . . . (4)

and that we use the equivalence (3) along with (4) to conclude,

that (2), when interpreted as a statement of  $S_1$ , is true (5)

Now in arguing thus, we accept that both (3) and (4) are true. That is, we presuppose the plausibility of  $S'$ . Hence we rely on a stronger formal system in order to establish the deductive incompleteness of the first with respect to one of its models. That is, to say that  $S$  is deductively incomplete is just to say that we accept  $S'_2$ . If we now further suppose that  $S'$  contains  $S$ —in the sense that all the theorems of  $S$  are theorems of  $S'$ —then  $S'_2$  satisfies the criterion of deductive incompleteness which I first proposed. For  $S'$  contains  $S$ ; and when interpreted in such a way that (3) and (4) are statements about  $S$ , it adds to the usefulness of  $S_2$  as determined by the initial purpose for which it was created. This purpose in terms of the present context, is to investigate the syntax of  $S$ . Another way of saying the same thing is to say that  $S$  and  $S'$  have a common model; viz. the syntactical model of  $S$ .

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## MALCOLM ON DREAMING

In his book, *Dreaming*, Norman Malcolm claims to have established that, "if anyone holds that dreams are identical with, or composed of, thoughts, impressions, feelings, images, and so on occurring in sleep, then his view is false"<sup>1</sup>. Stated most directly, Malcolm's argument for this conclusion is that a person's having a dream entails his being asleep, and any behaviour which would indicate experiences or activities would be in conflict with saying he is asleep. Malcolm repeatedly describes this supposed conflict in terms of contradiction (*ibid* pp 7, 36). Yet a strict notion of incompatibility is not adhered to. It is admitted that in many cases it is natural to say that someone manifests purpose or sensibility while asleep—e.g. when he tosses, mutters, shields his eyes or starts at sounds (*cf ibid* ch. VIII). It is suggested that the assertion that someone acts or suffers can, in such cases, be understood to reduce or diminish the assertion that he is asleep, but one does not know where this leaves the argument. The relation of sleep to behaviour indicative of purpose and sensibility needs to be reassessed.

It must be observed that Malcolm's idea of the criteria of sleep often appears to be rather superficial. It is not because a person is inert and unresponsive that he is pronounced asleep (*ibid* pp 22-23). He may be these by choice. As Malcolm himself suggests, it is only when it is wildly improbable that he would have chosen to be so, that his being inert would be grounds for insisting that he was asleep in opposition to his testimony (*ibid* pp 25-26). In fact, in pronouncing a person asleep, we are judging that orderly and efficient behaviour is not to be expected of him. The judgement is of capacities and their quality, and not of the occurrence of events. In one place Malcolm recognizes this explicitly, and a clear *non sequitur* results. In enquiring whether the sentence "I am asleep" is intelligible, Malcolm would ask about the person who uttered the words,

Was he aware of saying "I am asleep"? We need an outward criterion for determining this. Does he show a degree of alertness and knowledge of what he is doing that is normal in one who is awake? If the answer is affirmative, then he is not asleep. If negative, then he was not aware of saying anything (*ibid* p 16).

Obviously, "a degree of alertness and knowledge normal in one who is awake" is conclusive evidence that a person is awake. And individual pieces of alert and knowing behaviour would obviously constitute cumulative evidence of such capacities. They would not, however, constitute conclusive evidence. Nor would items of confused behaviour constitute evidence at all. There seems no reason, then, why one could not "mean the words" of the sentence, in so far as this would require being aware of saying them, for this is not a requirement that one be alert and knowledgeable. Much

<sup>1</sup> Norman Malcolm, *Dreaming* (London, 1959), Routledge and Kegan Paul, p 52

the same can be said of Malcolm's other contributing arguments. Hearing noises, feeling pain, arriving at conclusions and so forth are all things which one can do confusedly and without efficiently relating them to other elements of one's actual situation. Hence, they are things one may do in one's sleep.

The criticism just offered is divisible into two counts. First, there is a tendency in Malcolm's book to overlook the important distinction between capacities and occurrences. Second there is a tendency to ignore that a person who is adjudged asleep is being adjudged temporarily unfit or defective for the conduct of life, and that this may be through the erroneousness and disorder of his activity, as well as through the absence of any. The first mistake is indicative of the sources of Malcolm's equivocation in employing the notion of contradiction in his discussions of sleep and dreaming, and for this reason I shall write of it first. The second mistake is suggestive of positive arguments for regarding dreams as experiences, and I shall therefore write of it afterwards.

It is helpful, for gaining an understanding of Malcolm's way of dealing with occurrences and capacities, to generalize the arguments he offers. What is obtained is this argument. Having a dream entails being asleep, being asleep entails not being conscious or aware, and not being conscious or aware entails not having any mental experiences of any kind; therefore, dreams are not mental experiences.<sup>1</sup> None of the entailments in this sequence would be objectionable in a proper context and apart from the others. But the sequence is objectionable, because the notion of awareness or consciousness undergoes a change of sense between the second and third entailments. There are really two concepts of awareness or consciousness involved. A person must, logically, be conscious or aware of his experiences. Here, the concept of awareness serves only to emphasize that a person's experiences must be his. For this reason, a description of an awareness in this sense necessarily involves a description of an experience. Experiences being occurrences, awareness of an experience must be an occurrence also. In contrast, the next concept of awareness does not designate occurrences, but capacities. A person who is awake must, logically, be aware or conscious. This is merely to say that there is a "mental" counterpart to being awake. Since being awake is a matter of capacities, being aware in this second sense must be so too. Once the distinction between the two concepts is recognized, it is obvious that the awareness of a particular experience that is necessary for a person to have had that experience does not entail his being awake. He may have been aware of it without having been aware in general and, therefore, awake.

<sup>1</sup> Cf. *Dreaming*, p. 58. "Next, considering the impossibility of establishing that someone was aware of anything at all while asleep and the possibility of establishing that he dreamt, how can it follow from his remembering a dream that he was aware of the dream when he dreamt it?"

Many of Malcolm's points are argued as though corollaries of this mistaken argument. For example, Malcolm alleges that a person cannot be said to make assertions or judgements while asleep, without this implying that someone can be both awake and asleep at the same time, since he must be aware of what he asserts or judges (*ibid* p 36). But for a person to be aware of what he says while asleep would not necessarily be for him to be aware in general and, thus, awake. There is no contradiction. Similarly, a person may, while asleep, be said to manifest in ways other than speech that he is having experiences. For to suppose this would not be to suppose that he is conscious in general and not asleep after all. To attribute a particular expression, response or interest does not have that consequence. Yet these very points are crucial to Malcolm's position. For if it can further be established that reports of dreams may be referred to overt activity during sleep, discourse about dreams could duplicate without distinction the moves characteristic of discourse about waking impressions remembered.

To understand the kind of mistake Malcolm has made we need only to imagine a situation in which we would allow the distinction between occurrences and capacities to lapse. Suppose, for example, the way we would understand the notions of sleep and experience if our sole concern were the administration of a total and indiscriminating anaesthesia. If it were said that a patient was asleep, this would be understood to deny that he was in pain. No one could experience pain while asleep, and any pain would be conclusive that a patient was awake. It is because our ordinary talk of sleep relates rather to differentiated capacities for differentiated experiences that Malcolm's arguments can be avoided. This is what gives the distinction between capacities and occurrences its force. A single experience may be a conclusive test of a capacity described in the same terms. In the above example, where "asleep" means "anaesthetized", any pain would be conclusive evidence that a person is awake, because "awake" would be understood to mean "capable of feeling any pain". But our ordinary notion of being awake is not so narrowly conceived. As a result, it is only necessary to distinguish explicitly different types of experiences and capacities to refute Malcolm's arguments. A person may have been asleep, in that most types of observation and behaviour are not expected of him, but that would obviously not mean that he could have no thoughts or feelings of any sort.

Malcolm's reply at this point would be that the notion of being half-asleep has been surreptitiously introduced under cover of the unqualified term "asleep". It would be alleged that the foregoing deals only with "reduced" assertions that someone is asleep, where Malcolm is concerned with being fully asleep (*cf. ibid.* pp 29-31, 100). But the reply would assume a contrast that is badly expressed by the terms "half-asleep" and "fully asleep". It is not enough to save Malcolm's arguments, even nominally, that there should be



contrasting degrees of sleep. It would have to be possible to contrast all degrees of sleep with an absolute sleep. For the criterion of a sleep any less profound would be a person's susceptibility to minimal experiences, and, hence, would be useless for Malcolm's purposes. To support the contrast, it would be necessary to show cases of people saying that someone is asleep and meaning by this that he cannot have any experiences whatsoever. But we would know that this is what they meant only if there was some point in applying the term "asleep" to mark the threshold of mental life as such, without differentiation. I can think of no point to give substance to such a usage. We commonly meet threshold concepts in moral and legal discussion, indeed, wherever there must be a decision whether some rule is to apply or not apply. Being alive is such a concept. A person is considered alive if alive at all where it is a matter of his either possessing rights and duties or not possessing them. But in these practical contexts, we have no use for marking a mental annihilation apart from a physical. We are not so Cartesian.

Here it must be said that a kind of Cartesianism seems to invest Malcolm's thinking quite as much as the more obvious influence of Wittgenstein. Malcolm makes much of the following analogy in purportedly finding contradictions in views opposed to his :

It would not occur to anyone to conclude that a man is asleep from his saying "I am asleep" any more than to conclude that he is unconscious from his saying "I am unconscious", or to conclude that he is dead from his saying "I am dead". He can say the words but he cannot assert that he is asleep, unconscious, or dead. If a man could assert that he is asleep, his assertion would involve a kind of self-contradiction, since from the fact that he made the assertion it would follow that it was false (*ibid.* p. 7).

On reading this, the proof of the *cogito* at once comes to mind. What Malcolm has done is to take a set of first-person sentences too short to suggest immediately the uses they may have other than as threshold decision sentences. He asks us to read them as mental analogues of the apparent decision sentence, "I am not a person".<sup>1</sup> Then, on the assumption that a sentence implies in the same sense the conditions of its truth and the conditions of its assertibility, the sentences are found self-contradictory. Each move is characteristically Cartesian. Two replies to the reasoning are relevant here. First, it is never for us to decide, but only to learn, that we ourselves are to be spoken of as persons. Hence, we cannot contradict ourselves in so deciding. Second, only absolute sleep would preclude the possibility of being able to say that one was asleep. Otherwise, such a statement could be understood to except the capacity to make it, by a very common principle of accommodation.

The prevalent tendency of Malcolm's book is, then, to explain dreaming in terms which immediately oppose efficient waking

<sup>1</sup> Cf. *Dreaming*, p. 18. "In no circumstances can I wonder whether I am asleep, or be in doubt about it, any more than I can wonder or doubt whether I still exist."

behaviour to mental annihilation. In order to provide a positive argument for regarding dreams as experiences, it is only necessary to draw attention to cases of inefficient and disordered behaviour associated with sleep. It was pointed out earlier that a person's behaviour while asleep may legitimately be interpreted as indications of thought and feeling. If the thoughts and feelings reported as parts of a dream may be referred to the same time as the sleeping behaviour observed, there can be discourse about dreams which fully integrates the same kinds of evidence that are relevant to discussion of waking experience. We could refuse to accept a denial that the phenomena observed and reported were identical, in the same way that we would sometimes refuse to admit a distinction between waking pain manifested and reported. Cases of confused consciousness associated with sleep provide a standard for a *prima facie* identity of reference, and thus is all that is needed.

Suppose the case of a mother who reports, "I dreamt I was in forest and heard the baby crying, and I could not find him. Then I realized he really was crying, and I awoke." If we can take what the mother says seriously, it is clear that we can know when she dreamt she was in a forest. It was while the child was crying, an event to be verified in the ordinary way. Malcolm would reject this procedure because of his preoccupation with the notion of absolute sleep, but his arguments would have as little relevance as that notion. He would say that, if she really heard the child, she is mistaken to say she dreamt anything at the time; the forest must have been an hallucination. If, on the other hand, she really was asleep, she ought to say that she only dreamt she heard the child, although further cries did perhaps wake her subsequently (cf. *ibid.* pp 66-68). But there is nothing to suggest anything so pathological as an hallucination. Nor can we separate, and oppose, a dreamed hearing and a real hearing with any plausibility. Surely it would be better to conclude that part of what she dreamt turns out to have actually happened. What she actually heard, she heard in her dream.

There is nothing paradoxical about such interpenetration of dreams and waking life. It will seem so only in the course of an attempt to interpret it physically—i.e. spatially as well as temporally. Understood physically, the expression "in a dream" can be made out to involve a perverse admission that dreamed experiences do not occur, since they cannot occur at real places. This is what Miss MacDonald did, concluding that the notion of any kind of interpenetration was absurd.<sup>1</sup> It will not appear absurd if temporal and spatial interpenetration are carefully distinguished. It is true that there are no real counterpart objects and spaces for those dreamt, as there are for the objects and spaces of an hallucination. There is no "context of real objects". But cases of temporal interpenetration still provide a sense in which dreams can occur in a context

<sup>1</sup> Margaret MacDonald, "Sleeping and Waking", *MIND*, April, 1953

of real happenings. This sense is independent of the notion of real objects, since we can speak intelligibly of what seems to be the case without making reference to what seems to be other than it is.

The temptation to spatial interpretation removed, the expression "in a dream" can be seen as an idiom entirely appropriate to the treatment of reports of dreams as accounts of experience. What is indicated by the expression is the way an account of a dream may contain several items. In cases of interpenetration, an account will contain both items of fact and items of fancy. What unites them is the personal point of view of the narration as a whole. Such continuity exists alongside the discontinuity between elements of fact and fancy, just as it exists in stories of waking experience uniting assertions of what actually occurred and assertions of what only seemed to occur. "I dreamt" is very like "I thought" when used to preface such stories. In contrast, it is quite unlike "Once upon a time". Fiction—even first-person fiction—does not turn out to be history when it turns out to correspond to facts. But the account of a dream does, much as the report of a waking person who mistakenly distrusts his senses. Take the case of a man who says to his wife at breakfast, "I decided we should give up drinking. While you were asleep, I poured all the liquor down the drain." If this alarms her, he can deny his story in different ways. He can say, "I'm joking. I made it up." Or he can say, "I mean that's what I dreamt I did". Now, if in fact the bottles are still in the cupboard, and if they still contain the liquor they contained the night before, these two things may be practically indistinguishable. But suppose the shelf is empty, and that the dustbin is full of the empty bottles. Then, if he had said that he had made up the story, his wife would be completely in the dark as to what happened, provided she believed him. But if he had said that he dreamt the events he told her about, she would at once have a very good idea just what happened. "You didn't just dream it," she would say, "you did it and you remember doing it." In disagreeing with the characterization of an account as that of a dream, one moves at once to characterizing it as a report of actual occurrences in which someone was actually involved.<sup>1</sup> Only the dream denomination is withdrawn. The personal point of view of what was thought to be an account of a dream is not displaced on finding it is not. It stands as an account of actual thought, feeling and activity. Where a report is qualified at the outset in terms which deny that what is recounted even seemed to occur, this would not be natural. It would not be natural if the sleep associated with dreaming were understood to be absolute. It is evident that it is not so understood.

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<sup>1</sup> Cf. "Sleeping and Waking", p. 98. "There are no entailments between the statements in these two uses." This is, of course, true, but there are other logical relationships at least as intimate.

## THE FALLACY OF COMPOSITION<sup>1</sup>

In this paper I wish to point out some mistakes in the way in which the fallacy of composition has been characterized. In discussions of the composition fallacy, two points are frequently made. First, the composition fallacy is classified, along with equivocation, division, amphiboly, and accent, as a fallacy of ambiguity. Second, it is distinguished from the other fallacies of ambiguity by indicating its peculiar form of inference. Here it is usually said that the fallacy of composition makes an inference of the form: all the parts or members have this property, therefore, the whole or class has this property. I shall begin by discussing a difficulty connected with the description of the composition fallacy in terms of a certain form of inference.

Consider this statement of the composition fallacy: "The first type of *composition* fallacy is committed when one infers that a whole has a certain property from the premiss that every constituent part of that whole possesses the property in question" (Copi, *Introduction to Logic*, p. 47). Copi distinguishes this first type from a second type of the composition fallacy—namely, when an inference is made from properties possessed by the members of a class or collection to properties possessed by the class or collection as such. The distinction is between parts-whole and members-class.<sup>2</sup> Examples of the fallacy of composition (parts-whole type) are (1) all the parts of this machine are light, therefore, this machine is light (2) all the parts of this machine are small, therefore, this machine is small. It is clear that (1) and (2) are fallacious arguments. Moreover, both conform to the characterization Copi gives of the composition fallacy. In each case it is inferred that the whole has a certain property from the premiss that every constituent part of the whole has that property. Letting  $x$  range over wholes and  $\phi$  ranges over properties, a form of this inference is: all the parts of  $x$  have  $\phi$ ; therefore,  $x$  has  $\phi$ . What Copi is saying is that every argument employing this form of inference commits the fallacy of composition. But it is a mistake to think that every inference of this form is fallacious. Consider the following examples: (3) all the parts of this chair are brown, therefore, this chair is brown (4) all the parts of this desk are made of metal, therefore, this desk is made of metal. (5) all the parts of this object are located in space, therefore, this object is located in space. In (3), (4), and (5) we infer that the whole (chair, desk, object) has a certain property (being brown, being made of metal, being located in space) from the premiss that every constituent part of the whole has that property. Thus all

<sup>1</sup> This paper was presented at the meeting of the Western Division of the American Philosophical Association in May 1960.

<sup>2</sup> I have selected Copi's statement of the composition fallacy for two reasons. First, it does what other discussions fail to do, namely recognize explicitly that the fallacy has a parts-whole type, as well as a members-class type. Second, it is more suitable for my purposes since I intend to limit this discussion to the parts-whole type of the fallacy of composition.

three examples conform to the characterization of the composition fallacy. But would we want to say that (3), (4), and (5) are instances of the fallacy of composition? Instead, would we not deny that they are fallacies at all? It is logically impossible for all the parts of  $x$  to be brown, if  $x$  is not itself brown. It is logically impossible for all the parts of  $x$  to be made of metal, if  $x$  is not itself made of metal. And it is logically impossible for all the parts of  $x$  to be located in space, if  $x$  itself is not located in space. If this is so, (3), (4), and (5) are valid arguments, and, for this reason, not fallacious. Hence, it is a mistake to say that a fallacy is committed when one infers that a whole has a certain property from the fact that all the parts have that property. (3), (4), and (5) make such an inference but are not fallacies.

But perhaps there is an error in this analysis. Is it really *logically* impossible for all the parts of a chair to be brown if the chair is not itself brown? If we answer in the affirmative, as I have done, are we not presupposing the empirical truth that when chair-parts of a certain colour are connected together to form a chair no mysterious process of colour transformation occurs such that the chair has a colour different from the colour the parts had prior to their being connected together to form the chair. That is, is it not logically possible that chair-parts be of a certain colour but that when combined to form a chair a transformation occurs such that the chair is a different colour? And if such is logically possible, then is it not still a fallacy to infer that  $x$  has  $\phi$  if all the parts of  $x$  have  $\phi$ ? But notice what has happened here. We have separated the chair-parts from the chair. We talk about their colour *before* they are connected together to form the chair. And I suppose it is logically possible that before being connected together the chair-parts could be of one colour but when connected together to form the chair a colour transformation occurs such that the chair is of a different colour. But this is irrelevant to the argument. We want to know whether all the parts of *this* chair logically can be of one colour if the chair is of another. That is, we are talking about the colour of the chair-parts that already are connected together to form a particular chair. Can all the parts of this chair—when connected together to form this chair—be brown if the chair is not brown? It is this point that I am asserting is logically impossible. And, if this assertion is correct, it is not a fallacy to assert that this chair is brown from the premiss that every constituent part of this chair is brown. Hence, our conclusion that (3), (4), and (5) are valid arguments, and not fallacies, remains intact.

Another way of getting to this conclusion is to ask how we would prove the validity of (3), (4), and (5) in formal logic—for example, in the first order functional calculus. Consider (3), all the parts of this chair are brown, therefore, this chair is brown. As it stands the inference is technically invalid in the functional calculus. This is because a necessary premiss is missing—namely (3a) ( $x$ ) (if all the parts of  $x$  are brown,  $x$  is brown). Given this additional premiss (3)

can be proved valid in the first order functional calculus. Now, apart from the additional premiss (3a), should we consider (3) a valid argument? This will depend on how we view (3a). If (3a) is a necessary truth it is impossible for all the parts of this chair to be brown if this chair is not itself brown. And if that is so then (3) is, by definition, a valid argument, not a fallacy. The situation here is similar to what we would say of the following argument:  $a$  is older than  $b$ , therefore,  $b$  is not older than  $a$ . I think we would want to say that this is a valid argument. But to prove this argument valid in the first order functional calculus we have to add the additional premiss that the relation of 'being-older-than' is asymmetrical—i.e.,  $(x)(y)(Oxy \supset \neg Oyx)$ . I regard this additional premiss as a necessary truth. It is logically impossible for  $x$  to be older than  $y$  and  $y$  to be older than  $x$ . The impossibility is due to the meaning of the expression 'being-older-than'. Because this added premiss is a necessary truth we do not hesitate to say that the original argument is valid. For if  $(x)(y)(Oxy \supset \neg Oyx)$  is a necessary truth it is impossible for  $a$  to be older than  $b$  if it is false that  $b$  is not older than  $a$ . If so, the argument is valid. Similarly, (3) is a valid argument if (3a) is a necessary truth. Granted our customary meanings of the expressions 'an object', 'all the parts of this object', and 'being brown', would we not be contradicting ourselves if we said 'all the parts of this object are brown but this object is not brown'? What could we possibly mean if we said 'all the parts of this object are made of metal but this object is not made of metal'? Similarly, given what we customarily mean by 'located in space', are we not contradicting ourselves if we say 'all the parts of this object are located in space but this object is not located in space'? It seems to me that in each case we would be involved in a contradiction. Hence (3a), (4a), and (5a) I regard as necessary truths. If this is correct then (3), (4), and (5) are valid arguments—from which it follows that it is incorrect to characterize the composition fallacy as an inference that a whole has a certain property from the premiss that every constituent part of that whole has that property.

I have argued that it is not always a fallacy to infer that a whole has a certain property from the premiss that all of its constituent parts have that property. If I am right then we cannot say that the fallacy of composition is committed whenever we made an inference of the form: all the parts of  $x$  have  $\phi$ ; therefore,  $x$  has  $\phi$ . The validity or invalidity of the inference seems to depend on what property we substitute for  $\phi$ . If we substitute 'brown' (or any colour word) we will get a valid inference. However, if we substitute some relative term such as 'light' or 'small' the inference may be invalid. But why is this so? What has happened in (2) 'all the parts of this machine are small, therefore, this machine is small' that has not happened in (3) 'all the parts of this chair are brown; therefore, this chair is brown' such that (2) is fallacious but (3) is valid. Is it not the case that 'small' is used *ambiguously* in (2),

whereas 'brown' is not used ambiguously in (3). 'Light' and 'small' are relative terms; whereas, 'brown' and 'made of metal' are not. A small part and a small machine are in different categories. A large machine, as machines go, could be made of many parts all of which are small, as machine parts go. Thus 'small' is used ambiguously in (2). The criteria for determining smallness of machine-parts are different from the criteria for determining smallness of machines. But the criteria for determining brownness of chair-parts are not different from the criteria for determining brownness of chairs. This leads us back to the classification of the composition fallacy as a fallacy of ambiguity. What this means is that the fallacy of composition is *due* to ambiguity. That is, some word or phrase, occurring in premiss and conclusion, means one thing in the premiss and another thing in the conclusion. Is this not why (2) is fallacious? 'Small' means one thing in the premiss (when applied to machine-parts) but something else in the conclusion (when applied to machines). Hence, perhaps we can remedy the situation by saying that the parts-whole type of composition fallacy is committed when (a) one infers that  $x$  has  $\phi$  from the premiss that all the parts of  $x$  have  $\phi$ , and (b)  $\phi$  is used ambiguously—i.e.,  $\phi$  means one thing in the premiss and another thing in the conclusion. This way of putting it means that every instance of the fallacy of composition is due to ambiguity. Something like this is suggested by Cohen and Nagel's discussion of the composition fallacy. Their description is not open to the same criticism that I have levelled at Copi. For, they do not claim that it is *always* a fallacy to infer that a whole has a certain property from the premiss that all the parts have that property. Instead they claim only that such an argument *frequently* is fallacious. Apparently, they think it will be fallacious when the property-word is used in one sense when applied to the parts and in another sense when applied to the whole. Thus in giving a reason why the composition inference is fallacious they say, "For the same word may have a different significance when applied to a totality than it has when applied to an element" (*An Introduction to Logic and Scientific Method*, p. 377). Again, in speaking of what they call the "semi-logical or verbal fallacies"—which include the fallacy of composition—they say, "These all seem to conform to valid forms of inference, but on careful examination are seen not to do so—the appearance being due to ambiguity . . ." (p. 376). These quotations indicate that the parts-whole type of the fallacy of composition is committed when and only when (a) the parts-whole inference is made and (b) the property-word is used ambiguously—i.e., in one sense when applied to the parts, but in another sense when applied to the whole. Thus, Cohen and Nagel might claim that (3), (4), and (5) do *not* commit the fallacy of composition because even though they involve the parts-whole inference the words 'brown', 'being made of metal', and 'being located in space' have the same meaning when applied to the parts as they do when applied to the whole. But

examples (1) and (2) do commit the fallacy of composition because they make the parts-whole inference *and* the words 'light' and 'small' are used ambiguously. I wish to make two remarks about this position. The first is simply a warning. It seems to me that a relative term like 'small', 'light', or 'strong' can be used unambiguously in a parts-whole inference so as to yield a valid argument. At least, this seems to be the case with the word 'strong'. Consider this example (6) all the parts of this chain are strong, therefore, this chain is strong. I think it is logically impossible for all the parts of a chain to be strong if the chain is not itself strong. Presumably the parts of a chain are its links. My suggestion is that the criteria for determining the strength of links are no different from the criteria for determining the strength of the chains composed of those links. If a given link is only so strong—i.e., can hold only so much weight or stand only so much pressure—then the chain will be no stronger than that link. Thus is what it means to say 'A chain is no stronger than its weakest link'. Now if all the links of this chain are strong in comparison with the same size links of other equally long chains, then it is necessarily the case that this chain will be strong in comparison with the other chains. Hence, the mere presence of a relative term in a parts-whole inference does not mean that the argument is an instance of the fallacy of composition. Cohen and Nagel could accept this point. All they need claim is that the fallacy of composition is committed when and only when we make the parts-whole inference and use the property-term ambiguously.

The second point I want to make about this analysis of the fallacy of composition is that it is not true that every case of the fallacy of composition is due to ambiguity. For I think we can commit the composition fallacy even though the property-term has the same meaning when applied to the whole as it does when applied to the parts. Consider this example (7) all the parts of this figure are triangular, therefore, this figure is triangular. This argument is invalid and I think we would want to list it as an instance of the fallacy of composition. But the word 'triangular' does not mean one thing when applied to the parts of the figure and something else when applied to the figure itself. It has precisely the same sense when used in the conclusion as it does when used in the premiss. Now if we call (7) an instance of the fallacy of composition it is surely a mistake to suggest, as Cohen and Nagel, Copi, and most others who discuss the point do suggest, that the fallacy of composition is due to *ambiguity*. In fact, it seems a mistake to classify it as a fallacy of ambiguity. Some instances of the composition fallacy seem to be due to ambiguity. But, as (7) shows, not all instances can be explained in this way.<sup>1</sup>

<sup>1</sup> In commenting on my paper Mr Charles E. Caton of the University of Illinois pointed out that it is not even true that *every* parts-whole inference involving ambiguity in the property term is fallacious. His example is: all the parts of this chair are small; therefore, this chair is small. Where



What I have done so far in this paper is to consider two ways of analysing the parts-whole type of the fallacy of composition. The first way, suggested by Copi, is to say that we commit the fallacy whenever we make the inference all the parts of  $x$  have  $\phi$ , therefore,  $x$  has  $\phi$ . I rejected this analysis on the grounds that the validity or invalidity of the arguments involving this inference will depend, in part, on what properties are substituted for  $\phi$  (3), (4), and (5) infer that a certain whole has a certain property from the premiss that all the parts have that property. But (3), (4), and (5) are valid arguments and not instances of the fallacy of composition. The second way of analysing the parts-whole type of composition fallacy is to say that we commit the fallacy when and only when (a) we infer that the whole has  $\phi$  from the premiss that all its parts have  $\phi$ , and (b)  $\phi$  is used in one sense in the premiss and in a different sense in the conclusion. I rejected this analysis on the grounds that not all instances of the fallacy of composition involve such a shift in meaning (7) was given as an example of an argument that commits the fallacy of composition but whose fallaciousness is not due to ambiguity. This rejection, if correct, means that it is a mistake to classify the fallacy of composition as a fallacy of ambiguity.

I wish to make one brief comment in the direction of a more adequate analysis of the fallacy of composition. If my arguments are good ones, it is clear that we can have no formal or general characterization of the fallacy of composition. What we can say is that the fallacy of composition is committed in certain, but not all, arguments which make the inference all the parts of  $x$  have  $\phi$ , therefore,  $x$  has  $\phi$ . Then we must simply display and discuss cases which are fallacious, such as (1), (2), and (7), and cases which are valid, such as (3), (4), (5), and (6).

In this paper I have restricted myself to the parts-whole type of composition fallacy. Two questions seem relevant here. (1) Does the argument of this paper extend to the members-class type of composition fallacy? (2) Can these remarks be extended to cover the whole-parts type of the fallacy of division? I think the answer to the first question is largely negative. At least, I am unable to discover any counter-examples corresponding to (3), (4), and (5) in the parts-whole type. This may be due to the difference in type between a class and its members. To the second question, I am unsure what the correct answer is. Is it always a fallacy to infer that all the parts of  $x$  have  $\phi$  from the premiss that  $x$  has  $\phi$ ? I am inclined to think that it is not. But it seems more difficult to provide counter-examples when the inference is from whole to parts.

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we understand the parts to be the legs, seat, and back, it is not fallacious to infer that the chair is small from the premiss that all its parts are small. This point is a further indication that the analysis of the composition fallacy we are considering is mistaken.

## "ACTION" AND "CAUSE OF ACTION"

IN his article, "The Ascription of Responsibility and Rights",<sup>1</sup> Professor H. L. A. Hart distinguishes between two theories of human action. The "old-fashioned" theory is the view that an action is distinguishable from a physical movement of the body because it has correlated with it as its psychological cause a mental event called an "intention". The "modern" theory translates a statement that an action has been performed into the assertion of a categorical proposition about the bodily movement *and* a general hypothetical proposition stating the causal consequences which would have resulted had a different choice been made. Both theories Hart rejects as inadequate. According to him the concept of action is an ascriptive, not a descriptive, concept. A physical movement becomes an action in so far as either (1) the actor is held responsible for it, or (2) except for certain excusing conditions would be held responsible for it.

In opposition to the "old-fashioned" psychological criterion and in support of his own, Hart advances the argument that the psychological criterion fails to explain the line we draw between actions which are accidental and other cases involving neither intention nor responsibility (p. 163). He cites the case of a man who aims at a post and misses it because the wind carries off the bullet, which hits a man. He is said to have shot the man accidentally. In the other case, the man aims at and hits the post, but the bullet ricochets and hits another man. According to Hart, this would not be an action at all. We make the distinction, yet in neither case was there an intention to bring about what in fact occurred.

I do not wish to try to defend the older psychological theory of mental intentions, but to point out that Hart's own theory does not adequately account for the legal ascription of responsibility in cases of this sort, *i.e.* cases of accidents and other cases where legal responsibility is neither ascribed, nor ascribed but excused. Legal cases can be cited to show that men are often held legally responsible for accidents, a fact which Hart surely would not care to deny, but which, if admitted, has the philosophical importance of establishing that on his theory there can be no distinction between intentional acts and accidents. Secondly, it can be shown that there are cases in which the ascription of legal responsibility is wholly excluded, but which nevertheless presuppose a concept of action. If this be so, then Hart's theory seems to lead to the absurdity of denying a difference between legally responsible actions and actions for which no one is legally responsible.

Consider first the type of case in which a person is held legally responsible for his accidents. One case,<sup>2</sup> very much like the one

<sup>1</sup> Reprinted in *Logic and Language*, 1st ser., ed. by Antony Flew (Oxford Basil Blackwell, 1951), pp. 145-166.

<sup>2</sup> *Burlington and M. R. Co. v. Westover*, 4 Neb. 268.

Hart himself cites, involves a person who sets a fire which is then blown out of control beyond the intended area and causes damages to another. The rule of this illustrative case which many American courts follow is that a defendant is liable for all the *direct* consequences of his negligence *even though they were unforeseeable*. This case is easily distinguished from, say, the case of a person who deliberately sets fire to his neighbour's property. The difference would be that between an accidental occurrence and an intentional act. And on Hart's theory, since legal responsibility is ascribed in both cases, they would both be "actions". The question, however, is whether Hart can distinguish them. He cannot distinguish them by reference to the factual circumstances which, as he says, "support" or "are good reasons for" the ascription of responsibility, since in the cases cited these might be observationally the same. And if we deny meaning to "intention" apart from "responsibility", there is seemingly no way in which he can distinguish the one action as an accident and the other as a deliberate act.

The second type of case I wish to consider is one in which an action is admitted but in which responsibility is neither ascribed, nor ascribed but excused. On Hart's theory there should be no such cases. Yet consider what is involved in a case where a demurrer to a plaintiff's complaint is made and judicially affirmed. A demurrer is merely a legal way of saying that even if the charges of the complaint that a certain act took place are true there are no grounds for holding the defendant legally responsible. Legally speaking, the demurrer alleges that there is no "cause of action." In one such case<sup>1</sup> involving a demurrer the judge said, "It is clear from the most casual inspection of the complaint that it does not state a cause of action. No act of negligence on the part of the defendant is alleged. It is alleged that the [street] car which struck the plaintiff was running at the rate of six miles an hour; also that the plaintiff heard no bell rung on the car; but there is no allegation that six miles an hour was an improper or unlawful rate of speed, or that no bell was in fact rung."

Like the second case Hart cites, that of aiming at a post and hitting it, this case analogously involves the accomplishment of an aim, namely, that of the conductor in running his street car along its tracks. In both cases someone is incidentally injured. In Hart's case too, a demurrer would be in order and would probably be upheld. In other words, there would not be a "cause of action" in either case. But is there no concept of "action" involved? Surely someone shot the bullet and the conductor ran the street car. If these are not actions then what sense does the demurrer have? How can one admit, for the sake of legal argument, the existence of such actions, and yet be understood to mean, not merely that the claim has yet to be proved, but that what occurred was not even an action? Can we mean "action" and yet not mean "action"?

<sup>1</sup> *Lvdecker v St Paul City Railway Co*, 61 Minn 414, 63 N.W 1027

Hart might of course claim that since action is a defeasible concept, the concept of action presupposed by the demurrer is "defeated" by the Judge's sustaining of the demurrer. This, however, overlooks the fact that a demurrer is a refusal to defend against the charges, and so is not in the same category as defences such as mistake, provocation, insanity, and the like

Though Hart's theory fails, I believe, to distinguish between legally responsible actions and actions for which no one is legally responsible, it does establish this much, namely, that there can be no concept of a "cause of action" unless an ascription of responsibility is involved. No doubt "cause of action" is what he has interestingly called a defeasible concept. That the concept of "action" as found in statements of the form, "He did it," are also defeasible is, however, extremely doubtful

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## ON A GOOD-EVIL ASYMMETRY

In general we are inclined to assume that any decision on the question whether a given action or motive be good or evil requires ultimately an appeal to a (possibly 'higher') principle that involves, amongst others, these very terms good, evil. In other words, we believe the field of ethics to be logically autonomous

It would, therefore, be of considerable interest if, on the contrary, we could point to at least some cases where an 'objective' assignment of the terms 'good' and 'evil' respectively to two alternative courses of action or motives might be possible, by appeal to an analytical principle. This will be possible if we can construct some case whose formal structure alters when we substitute one alternative for the other; in other words, if we can point to some logical asymmetry between the alternatives. The result of such a substitution could then serve, not indeed as a definition, but as a test of good as distinct from evil, in the same sense that a spectrometer recording of a line is not a definition of, but a test for, a certain colour.

These remarks are prompted by reflecting on the following situation pictured in a humorous magazine: the father, one of a well-known family of wicked monsters, is showing his daughter the family album, remarking "This is your Uncle Albert, of whom it may truly be said that he left this world a worse place than he found it."

Now, 'what is wrong with this picture?'

We may take as implied in the story that evil is held up as supreme single principle in this family, and that the father is citing this uncle as an example for his daughter to follow ("Following the example of your uncle will be to your advantage")<sup>1</sup>

Yet there seems to be an inconsistency in the father's presenting evil as supreme guiding principle. I suggest the inconsistency lies in this: Why is the father teaching the daughter (as it happens a lesson in evil)? What is the guiding principle under which the teaching itself takes place at all? If the father is consistently evil, presumably he is misleading the daughter to her downfall, and his teaching is, in fact, false. That is, it is not, in fact, advisable for the daughter to follow her uncle's example. In that case, the father would be lying deliberately (and lies are in principle detectable by non-ethical means). If, however, the father is teaching the daughter for her good, his actions are inconsistent to the extent of his relations with his daughter. There is at least a cell of impurity of good in the network of his evil actions.

<sup>1</sup> While we are free to formulate a hypothetical situation I suggest that we are actually formulating a teaching situation fairly realistically: *persuasive* teaching of this kind, after all, implies "I am teaching you for your own good." Persuasive teaching, incidentally, is strengthened if the teacher serves as an example.

Thus we claim that the following thought experiment discriminates between 'good' and 'evil'.

A teacher teaches his pupil "Always being X will be to your advantage"

Substitution of 'evil' for X implies a situational structure that either involves a lie, or an inconsistency in the actions of the teacher in the sense explained above. Substitution of 'good' does not of necessity imply a situational structure involving either a lie or an inconsistency in the teacher's actions

Thus it is suggested that evil can be detected here by the impossibility of building a consistent network purely of evil, while there is no obvious reason why it should be impossible to build a consistent network of good (uncle and father good, daughter instructed to be good likewise). We may think of the analogy of certain unit cells that can serve as building bricks for indefinitely extended crystal lattices, while other elementary structures cannot be used for this purpose.

This reference to 'consistency' reminds us of a distinction hinted at by Kant, who points out that some actions are of such a kind that their maxim could not even be *thought of* as a general law of nature, while others are such that it is merely impossible to *wish* that their maxim be raised to the generality of a law of nature<sup>1</sup>

While there are few clear-cut examples of the extreme case (Evil type A) where the ground for rejection of a maxim is the *logical* impossibility of making it into a universal law ("I ought to be richer than everybody else"), the example discussed above is at any rate nearer to this extreme than to the other (Evil type B), where the ground for rejection is merely the impossibility of *wishing* the maxim to be a universal law.<sup>2</sup>

H. R. Post

<sup>1</sup> *Grundlegung zur Metaphysik der Sitten* p 424

<sup>2</sup> We may mention an example of yet another kind: Consider a motorist who persistently makes a loud honking noise to cause others to get out of his way. This is not type A evil. it is logically *possible* to imagine all present motorists to follow the same policy. Yet this would defeat the original *purpose*, in that the signal as warning would be drowned by the universal noise. it would, in fact, no longer be a signal. This is a stronger situation than type B which merely requires that the motorist cannot wish for this outcome

## SCOBIE ON "THE IDENTITY OF A WORK OF ART"

IN a recent article, "The Identity of a Work of Art" (MIND, January 1959), Joseph Margolis discussed such problems as the sense in which a poem and its translation are the same poem, and the sense in which an original play and its "interpreted" performance share an identity. W. D. L. Scobie (MIND, April 1960) challenged the relevance of some of Margolis' remarks to theories of art and, consequently, the correctness of Margolis' assumptions. I am interested here only in examining the confusion which underlies Mr. Scobie's argument.

Mr. Scobie apparently holds that it is always absurd to speak of an identity between such above mentioned pairs. He says that there is "no aesthetic relationship" between an original work of art and an interpretation which alters it. What is meant by the term "aesthetic relationship" is not made clear, and I am unable to attach a meaning to what appears to be a central notion. In any event, there can be no *aesthetic relationship* between the work of art and its "interpretation" because any deviation from the score, stage directions, etc., "must be an error". Mr. Scobie's charge of error is a very serious one for the simple reason that many people devote considerable time to making such changes. The charge is also incorrect.

Mr. Scobie attempts to justify his charge of error by citing the following examples. Although the playwright or the composer requires "the interpretive talents of others, his intentions, as made explicit in the final script, are the prime notation from which any deviation must be an error". He also objects to the casting of statues in metal from original stone works of art—"the artist creates by attacking a block of stone with a chisel and the resulting surfaces are conceived in terms of stone, and this is the artist's intention". "The composer's manuscript is the prime notation of a work of art, any deviation from it is a deliberate disregard of the intention of the artist." From "any deviation must be an error" and "any deviation . . . is a deliberate disregard of the intention of the artist", it seems plausible to conclude that Mr. Scobie thinks deviation is an error because it violates the artist's intentions. The supreme value of the intention of the artist is the central notion of his discussion. It has been rightly argued that concern with the artist's intention is a fallacy. Why must we be so reverent toward the intentions of the artist? In other areas of life we often feel free and in some cases bound to disregard the intentions of men. Also it is notoriously difficult and often impossible to ascertain the intentions of the artist. Even in those cases in which the intentions are made explicit, why must one be bound on pain of error to follow all (or even any) of them? Rigid conformity to such specifications is a form of ancestor-worship or confusion of historical value with esthetic value. I am not advocating a policy of always changing a work, but if an alteration seems like an improvement, there is no reason not to try it. It will succeed or fail according to the talent of the person who

alters the work. There is no reason to suppose that earlier talent is always better than present talent. In decrying what is often done in music and the theatre, Mr. Scobie writes, "No one would dream of altering in reproduction the colours of a painting, or the position of a limb of a sculpture, because they thought it looked better their way . . ." There is no theoretical reason for not making such alterations. However, the practical reason is obvious—most people would rather buy a reproduction of a well-known, recognizable painting (however bad) than a changed one (however improved).

The emphasis on intention confuses understanding the creative act with esthetic enjoyment. If one is interested in the dynamics of the creative act, then the artist's intentions are important for one's inquiry. But the artist's intentions are irrelevant to the esthetic value of the work of art, it must stand or fall on its own merits. Mr. Margolis' analysis is concerned with works of art from the point of view of esthetic enjoyment, and not with their origins (the creative acts). Mr. Scobie's confusion of the creative act with esthetic enjoyment is borne out in his concluding remarks, where he asserts that Mr. Margolis' analysis is useful in "discussing the position of the artifact" but is "no help at all in establishing the identity of a work of art . . .". An artifact is apparently distinguished from a work of art in that the latter involves the intentions of the artist. But as indicated already esthetics and criticism are concerned only with the work which is presented and not with what was intended. What is presented is what Mr. Scobie calls an artifact and what Mr. Margolis calls a work of art.

One of Mr. Scobie's intentionistic remarks deserves note. If a lithographic copy is produced using colours and textures other than those intended by the artist, "the result is very clearly an artistic rape . . .". Artistic (creative) rape perhaps, but it might produce a beautiful child. Ethics and esthetics do not necessarily run parallel courses.

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## VIII.—CRITICAL NOTICES

*Thought and Action.* By STUART HAMPSHIRE. Chatto and Windus, London, 1959. Pp. 276.

BEHIND the scenes of the philosophical argument in this book, there is a director cum critic who utters a prologue, takes an occasional hand in the prompt box, gives a short speech at the end, and wrote the programme notes. This director is also the author. It is entirely appropriate that a philosophical author should comment on his own work in this way, but it is equally clear that in this case his activities have made the critic's task very difficult.

Presumably the two main sources of difficulty, which reviewers in the allegedly serious press were so strikingly unable to surmount, consist in the temptation to commit a fallacy of privileged position (whoever does something must best be able to describe it), and the (natural) occurrence in the author's remarks of references to contemporary linguistic analysis and traditional philosophy, which detonate reactions of loyalty or distaste with depressing regularity. Let us deliberately postpone any discussion of the programme notes until we have discussed the play, making only two general comments. First, a reviewer might more readily be forgiven in this case for believing that the author must, better than anyone else, know what he is doing, when it is recalled that about half the book is devoted to an analysis of the concept of knowing-what one is doing. Second, the book is of considerable originality and written in a way that is by no means easy to take in, quick reviews were, therefore, almost inevitably superficial. The same must, though in a different way, be true of any short review.

Hampshire sets out to discuss "certain familiar contrasts the contrast between that which is unavoidable in the structure of human thought and that which is contingent and changeable, between inner thought and its natural expression in speech and action; between that which confronts a man as the situation before him and that which is his own response to it; between knowledge and decision; between criticism and practice, between abstract philosophical opinions and the concrete varieties of experience". He hopes to trace the general connections between these contrasts and "to bring moral argument nearer to the philosophy of mind". For those with more specific interests, used to technical labels, it is apparent from close study of the text that comments are being made with great significance for phenomenism, atomism, idealism, the doctrine of innate ideas, Russell's theory of similarity, sense-datum theorists, introspection, libertarianism, the emotive theories, intensionality, the self, man's essential nature, behaviorism, dualism, relativism, Hegelian metaphysics, referring expressions, 'private languages', non-propositional knowledge, and the capacity of automata. There is almost no employment of the technical terms

just mentioned, however; indeed, the book is striking for its restriction to 'common English' (though "velleities" is a shade archaic and a "feeling of unpleasure" rather uncommon). Hence, philosophical relevance is often a matter of rather complex inference.

On the very first page of text we strike material, characteristic of the book, that is at once simple to read and hard to grasp. The page contains a dozen remarks, mainly about the nature of language, each of the kind which would today usually require between an article and a score of books of examine. "Reality and experience cannot be thought about unless we have rules that correlate particular groups of signs with particular recurrent elements in reality and experience . . ." What would be an example of such a rule? Why are they necessary? Can "recurrent element" be given a meaning without circularity, & without appeal to the recurrence of a certain sign-group in linguistic activity 'about' the element? Answers are not vouchsafed. We must either play along and hope eventually to judge these axioms by their consequences, or stick in our heels now and ask Blackwells for a refund. Page one being a little early for that, we continue. But, and it is a "but" that looms larger with every page for almost fifty pages, surely the *first* of those questions should be answered, surely we would benefit from some examples to elucidate such tricky concepts and claims. For those who are 'playing along', this makes it hard to tell whether one is playing along in the same game.

Many of Hampshire's conclusions in this book are so sweeping, however, that it is extremely difficult to see how any ordinary interpretation of them could salvage their truth. Here are two early examples, less questionable than many others, and scarcely bettered by still others. "It is also necessarily true that everything resembles everything in some respect" (p. 31). "If we speak of the same situation, or the same sensation, lasting through some period of time, 'same' could always here be replaced by 'exactly similar'." (p. 28). These seem clearly incompatible with normal usage and yet the second, at least, is clearly about such usage. On the other hand, the apparent exceptions that immediately come to mind could probably be accommodated by a slightly modified statement. The number ten and the last lamprey eaten by a king do not *resemble* each other in any respect. They are both things, but having an applicable common predicate is apparently not the same—in normal usage—as bearing a resemblance. The latter seems to require that the common property should be one in terms of which contrasts are normally or could usefully be made. This point is presumably similar to the point that identity does not, despite the attractions of the simplest logical analysis, require the total absence of distinguishing characteristics. However, it seems we could extend the concept of resemblance and say that everything has a resemblance to everything else without any immediately apparent disastrous consequence. It is simply a matter of recent logical experience that

such behaviour almost always *does* have disastrous results. It is better to amend the claim to "*almost anything can properly be said, in some context, to resemble any other given thing*". It appears that similar adjustments need to be made to most of Hampshire's very general claims, of which several further examples are discussed in this review. 'Playing along' will thus have to be taken to mean a spirit of forgiveness, not only for absent clarifications and proofs, but also for present inaccuracies provided they do not seem too serious.

To take a further instance, in the extensive discussion of the nature of language, there appears to be a special failure to consider the application of his general theses to mathematics. It is certainly a "language . . . in which statements are to be made and contradicted", but is by no means so clearly one to which his major thesis, that "we always need some criterion of identity" of the "object of reference", applies (pp 16, 17). What is the "object of reference" of a numeral or a variable or, a summation sign or alephzero? What is the cash value of saying that we have criteria of identity for the objects of reference of these terms? If it means that they have correct and incorrect uses, it is true but misleadingly put. If it means something more, it must be explained and justified. And, of course, similar puzzles arise about abstract terms in non-mathematical languages.

Hampshire moves from this linguistic thesis, as from many others, to an ontological conclusion. "I am in effect arguing that we must unavoidably think of reality as consisting of persisting things of different types and kinds . . . rather than of events or processes of different types and kinds" (p 17) (Not just our present reality, but *any* reality). But, it seems to me, on the same page, it is made clear that the term "thing" is being used so generally that the contrast with an event or process is vacuous. This contrast is said to depend on the "particular grammar that already distinguishes nouns and verbs in a particular way". This 'particular way' is not clarified, but it seems apparent that one either has or can introduce thing-terms within most languages to stand for processes, so that the grammar will not separate the two. (Of also "There are no criteria that anything must satisfy in order to be called a thing in the widest sense of the word" (p 25)).

I am by no means certain of the conclusiveness of these criticisms, but I cannot see how to avoid recording them as evidence of what appeared to one reader as the inconclusiveness of the author's argument in these early pages. Despite this inconclusiveness, and not because of it, there are many highly illuminating threads running through the early discussions, particularly about the status of classification and individuation, the inexhaustibility of truths, the essential function of the gap between appearance and reality, the illusion of ultimate elements of experience, and the differences between the sensory modalities. Many of these threads represent,

in a way not hitherto available, what I would judge to be the consensus of opinion amongst contemporary epistemologists of the so-called Oxbridge school. These threads are interwoven in a way that makes clear they are part of a single system, the fabric of Hampshire's thought, not stray items in a patchwork of ideas and articles from different authors. Some of the threads one knows from his other writings to be almost entirely of his own devising (*e.g.* the elusiveness thesis about "things"), but all of them are tied together in a way which is clearly his own.

As one proceeds through the book the impression is constantly confirmed that Hampshire has an extraordinary strong drive to get a perspective view of his own thought and that of his time, and that this struggle has led him to a considerable number of interesting conclusions and suggestions. A selection from these is to some extent a manifestation of personal interests, and is greatly handicapped by the necessity for short quotations, but is appropriate here before we go on to raise certain objections to other claims and themes of the book.

"If anyone claimed that he had discovered a people speaking a language in which only sensations were referred to and described, he would be claiming something that could not possibly be true. It must at least be possible in this supposed language for the speakers to discriminate one person from another, if they have the means to do this, they already have the means to discriminate one object in the external world from another" (p. 57).

"We are in the world, as bodies among bodies, not only as observers but as active experimenters. We could not ever be observers unless we were sometimes active experimenters and we could not ever be experimenters unless we were sometimes observers" (p. 53).

"I often cannot, in reflection or introspection, distinguish as separable episodes the thought of what is to be done from the actual doing of it. A philosophical dualism, which supposes that my history is analysable into two parallel sequences of mental and physical events, does not give a possible account of the concept of action" (p. 74).

"It has been generally recognised in recent philosophy that the relation of words and statements to facts is elusive and can never be stated in any simple and general terms. It has not been so generally recognised that the relation between words and actions is equally elusive, and that there are the same difficulties in dividing a human being's conduct into a set of namable actions as there are in dividing the perceived world into a set of namable facts" (pp. 120-121).

"The very notion of predicting what plans I shall form collapses into self-contradiction. Either I take into consideration the reasons that will influence me, in which case I am already engaged in forming a plan, or I somehow contrive to ignore the factors that will

influence me, in which case I cannot honestly profess any confidence in my own prediction" (p. 130).

"The actual use on a certain particular occasion of certain words, and the gesture of pointing that may perhaps accompany the words, have sometimes been taken to be all that is involved in the notion of referring to a particular thing. This is a mistake. The question 'Which of these did you mean?' is not the same as the question 'Which of these did you mention?'" (p. 200).

"The moral argument, even at this level of generality, would always contain appeals to experience, the actual living through situations, as the instances by which the otherwise abstract ideal of conduct must be tested. But the appeal to experience must itself pass through descriptions of the situations cited *What* there was in a particular situation, or in a particular course of conduct, that made it degrading has to be isolated in descriptions, if it is to be clear in my reflection" (p. 219).

On the other side of the ledger there are five kinds of entry. First, there is a continuation of the imprecise, unclear or incorrect remarks of which some examples have already been given. It seemed to this reviewer that these diminished in frequency. Second, there is a very considerable amount of material which does not appear to be carrying much philosophical weight. Hampshire refers to some of his remarks as truisms (p. 67) and one could hardly avoid the feeling that heavy pruning of the truisms would have benefited the form and force of the argument. Third, there is an increasing number of fairly unconvincing claims—examples will follow—about the nature of philosophy and the relationships between and justification of different parts of philosophy. Fourthly, there are several descriptions of Hampshire's own philosophical position and of the significance of his arguments which are open to some question.

And finally there seems to me to be a serious weakness in Hampshire's treatment of certain key problems about the title topic—the relationship of thought to action.

The minor intra-philosophical errors and excessive verbiage are not a fruitful subject for further discussion here, I take up only the discussion of his key topic. Hampshire argues with Spinoza that thought (and in particular, belief) is not a kind of action. He gives various arguments for this, including the observation "It would seem logically absurd to prohibit by law the holding of certain beliefs . . ." (p. 155). A longer argument depends on the premise, "... it is a necessary feature of anything that can be called an action that one might on occasion want to do it and also decide to do it and yet for some reason fail to achieve the result" (p. 157). He then argues that one cannot ever fail to believe something when one wants to, because one cannot ever *succeed* in believing something when (i.e. just because) one wants to. "If I was told that I could satisfy my desire to believe by turning my attention away from the

contrary evidence, I would not call my ensuing state, brought into existence by these means, belief" (p 157) Unfortunately, the example is not so simply dismissed. Religious people, perturbed by the problem of evil, are often told to "fix their eyes firmly on the Cross," i.e. to concentrate on the moral virtues of God rather than on the difficulties. And there is every evidence that this procedure is often effective. We do have this rather furtive kind of control over what we and others believe, and in this respect the contrast with action fails. The failure is serious for Hampshire because most of his argument about freedom and responsibility for action rests on similar points. If a man stands by while a friend drowns, "There is no sense in which he failed to try to rescue his friend; he simply did not try" (p. 182). But again the case is unpersuasive; often, to fail to try is simply not to try when in a position to try. The trying here, like the believing above, really is quite like a course of action, as far as responsibility goes.

We go on to consider responsibility for ignorance and the lack of mental and other skills; and here Hampshire underestimates the relevance of thought to (responsibility for an) action, while continuing to exaggerate the distinction between thought and action. On the first point, he thinks of responsibility for an action as wholly dependent on capacity. But it is also a function of knowledge. ". . . if I try to *predict* that I will not try (to perform some particular act in the future), and to give the grounds upon which my prediction is based, I shall find myself talking only of the difficulty that I would encounter in bringing myself to make the attempt" (p 187). Now surely one can predict that one will not try something (e.g. a jail-break) because one can now see overwhelming reasons for not trying it then, though it is *easy* within one's power? It is not *difficult* for me to try, but it would be *foolish*. I could *easily* try to break out, but—on such an important matter—I am perfectly certain I will not try.

Conversely, one can disclaim responsibility for an action by pointing out that one could not possibly have had any *reason* for doing otherwise. A rider of this point is that one is often responsible for actions which one could not *at the time* have avoided (e.g. a crash when driving drunk), if they were foreseeable possibilities at an earlier time when rationally preferable alternatives that *were* within one's power would have avoided these possibilities (e.g. not drinking, or not taking the wheel). It is thus *not* the case that responsibility refers only to what one can now or could hereafter do, but Hampshire misses this. "There is a sense in which a man is responsible for any [present] condition that he would be able to change if he tried. . ." (p 185). It must be "*would, or would have been able*" and "*if he had tried, and had been able to foresee*". Despite his admirable and powerful stress on the crucial importance of self-understanding, objective appraisal and intellectual analysis in the analysis of free choice and morality, he thus underrates the

role of reason in assessing responsibility. The simplicity of the major conclusions of this chapter (pp. 221-222) is damaged by these two flaws, his separation between the will and the intellect is both too strong and too weak.

On the nature of philosophy in general and his own work in particular, Hampshire has a negative and a positive thesis that require comment. He criticizes the procedures of contemporary linguistic analysis on several occasions. At the crucial points in philosophy, where whole approaches are in dispute, "... the method of examining established concepts, as they occur in contemporary speech, is inadequate. There is no way of showing that the idioms in common speech which point to one decision are to be preferred to the idioms which point to an opposite thesis. Even were there such a method of decision by reference to current linguistic usage, one could still ask oneself whether accepted contemporary usage is not tied to a disputable moral outlook . . ." (pp. 155, 156). This is surely a shallow criticism. If the idioms are in balance, then the next step in any linguistic analysis consists exactly in probing the logical connections between the two views and other—perhaps moral—positions, and then turning the criteria of usage to bear on these other positions. It is difficult to imagine what practitioners of "the method of examining established concepts, as they occur in contemporary speech" Hampshire could have in mind, if they do not include Austin—or Waissman or Wittgenstein, both of whom also explicitly stressed the great importance of untangling the logical connections between different components of an overall philosophical position. There are other places where he suggests that a limitation on linguistic analysis is its failure to consider the whole social context of a language and not just its inner structure (pp. 233-234), but this, one takes it, is precisely the message of the "language games" of the *Philosophical Investigations*.

Hampshire's conclusion about philosophy is that there is *no* way of deciding between internally consistent overall philosophical views and that we can be sure only of the fact that our views will not for long be deemed satisfactory. "No philosophy of mathematics, science, or art, however general and abstract it may be, can be eternally valid" (p. 243). The reason is simple. It is "not because previous philosophers have made mistakes in their analyses, but because the subject matter under consideration has changed" (*ibid.*). Now, since "it is possible to characterize philosophy itself as a search for 'a definition of man' . . ." (p. 232), and our understanding of man's nature is always changing, the same relativism permeates it all. Now there is a sense in which one could argue that science is inconclusive, *viz.* that it is logically possible for any empirical conclusion to be wrong, and empirically true that many are wrong. This would not support a relativistic view of scientific knowledge corresponding to Hampshire's relativism about philosophy roughly because in science we accumulate certainty faster than we

discover or can expect to discover error. Relativity theory might have to go, one day, but—to take one part of it—the relativistic refinements of Newtonian laws will have to continue to be recognized as (a) definite improvements on Newton, and (b) very very good approximations to the truth. Now philosophy is possibly in a stronger position: its problems are more general and hence less susceptible to overthrow by local changes in our knowledge—and much of it is concerned with logical rather than empirical analysis. Why should one not suppose that we are constantly and markedly improving our philosophical insights? Where are the examples of philosophical errors uncovered by new insights into man's nature, so that we may consider an inference to the existence and kind of future ones and see if we are worse off than the scientist? They are markedly absent, and absent too from the subsidiary arguments which lead to conclusions such as "A morality 'left to itself' will survive unquestioned only if it is insulated from any serious experience of art." (p. 244), from which he infers that reliable ethical standards are impossible because of creativity in the arts (pp. 247-248).

In case it might be thought that Hampshire is only arguing against the possibility of absolute certainty in philosophy and would be willing to accept parity with science, let me conclude with a quotation that rules out this interpretation: "The conclusions of philosophy itself are always within the domain of opinion and not of knowledge" (p. 255).

To summarize the book's special merits lie in its efforts to attain an overall view of philosophy and man, to relate philosophy to common concerns, and to analyze certain specific notions. Its demerits, in my view, include inadequate instantiation and substantiation of generalizations, unnecessary verbiage, and uncertain description of its own and other positions, but they do not offset the virtues.

The book is not a triumph of copy-reading. I detected errors on pages 9, 14, 23, 40, 95, 142, 154, 165, 177, 187, 197, 202, 204, 205, 229, 251, 253, and 271.

MICHAEL SCRIVEN

*Die Aristotelische Syllogistik* By GÜNTHER PATZIG. Pp. 207. Göttingen, 1959. 37s

THIS lucid work is a valuable contribution to the study of Aristotle's formal logic. Professor Patzig calls it a 'logico-philological' investigation, and he seeks to bridge the gap between Łukasiewicz's logical systematizing and the traditional philological treatments of Aristotle's syllogistic. He is obviously well-equipped for this task. I shall indicate the main contents of his five chapters and offer some criticisms. I shall necessarily omit reference to many good points



he makes and to the useful summaries he gives of the views of ancient Greek and modern European commentators

1. Chapter 1 asks what exactly a standard Aristotelian syllogism is. As Łukasiewicz has already shown (*Aristotle's Syllogistic*, pp. 1-3), it differs in almost every possible way from the traditional syllogism. It is formulated in an 'if . . . then . . . ' sentence, not as an argument;<sup>1</sup> it employs only term-variables and not concrete terms, instead of 'every S is P' it says 'P belongs to every S'. Patzig makes these points clearly and draws on more evidence than Łukasiewicz. He claims, and is to show in Chapters 3 and 4, that certain major problems about Aristotle's theory of syllogism can be solved only if one keeps in mind the standard *Aristotelian* formulation of syllogisms.

The main controversial question in Chapter 1 is the question why singular terms are excluded from Aristotle's theory of syllogism. In this connection both Ross and Łukasiewicz refer to *An. Pr.* A 27, 43a25-43, where Aristotle divides what there is into three classes: some things (categories) are possible predicates but not possible subjects, other things (individuals) are possible subjects but not possible predicates, other things can be subjects and predicates. Ross (*Analyt.*, p. 289) explains the exclusion of singular terms from syllogisms by referring to the remark Aristotle makes, at the end of this passage, to the effect that arguments and enquiries are chiefly concerned with items in the third class. Łukasiewicz, seeking a purely logical ground for the exclusion, points out (p. 7) that 'in all three syllogistic figures known to Aristotle there exists one term which occurs once as a subject and then again as a predicate.' So, he argues, 'syllogistic as conceived by Aristotle requires terms to be homogeneous with respect to their possible positions as subjects and predicates. This seems to be the true reason why singular terms were omitted by Aristotle'. Patzig remarks, against this, that in each of Aristotle's three figures there is at least one term which occurs only as subject (or as predicate); so that Łukasiewicz's point does not explain why Aristotle should have demanded homogeneity for all terms entering into syllogisms, nor therefore why he should have excluded singular terms. Patzig offers a different solution to the problem, as follows.

The chapter of the *Prior Analytics* quoted above is a preparation for the next chapter (c. 28), in which Aristotle gives procedures for proving conclusions of the four kinds A, E, I, O. For example, to prove that A is universally predicable of B one must find a term C which is universally predicable of B and of which A is universally predicable. Patzig says that in this section Aristotle assumes that every term possesses both possible subjects and possible predicates. For the procedures he recommends assume the following axioms: (a) every possible value of a syllogistic term-variable A has at least

<sup>1</sup> But see J. L. Austin in *MIND*, 1952, pp. 397 f. and A. N. Prior's *Formal Logic*, p. 116.

one genuine sub-ordinate term  $X$ , i.e. a term  $X$  such that  $A$  belongs to every  $X$  but  $X$  does not belong to every  $A$ , (b) every possible value of a syllogistic term-variable  $A$  has at least one super-ordinate term  $X$ , i.e. a term  $X$  such that  $X$  belongs to every  $A$  but  $A$  does not belong to every  $X$ . Now, Patzig continues, singular terms do not satisfy axiom (a) and categories do not satisfy axiom (b). Thus the reason why Aristotle confines syllogistic terms to items in the third class (items that can be subjects and predicates) is that only so will all terms satisfy the axioms presupposed by the proof-procedures given in *An. Pr.* A 28.

This explanation is open to three main objections. First, it seems a little unnatural to explain a central feature of Aristotle's syllogisms by appealing to procedures he recommends in A 28 for proving propositions. For had he incorporated singular terms into his syllogisms he could easily have amended the advice in A 28 so as to cope with such cases. Patzig says that he had to omit singular terms in order to safeguard A 28; one might rather argue that he wrote A 28 in the way he did because he had omitted singular terms. But this leaves us with the question why he had omitted them. Secondly, Aristotle does not say in A 28, nor would he wish to say, that every proposition is provable. The fact that a term failed to satisfy the axioms assumed by the proof-procedures of that chapter would explain the exclusion of that term from syllogisms only if it were also evident that an unprovable proposition could not occur anywhere in a syllogism. But this is not evident. Thirdly, Patzig's formulation of the presupposed 'axioms' is surely wrong. Aristotle says that to prove that  $P$  belongs to every  $S$  you must find a term  $M$  such that  $M$  belongs to every  $S$  and  $P$  belongs to every  $M$ . He does not say that you must find a term  $M$  with the further character that  $S$  does not belong to every  $M$  and  $M$  does not belong to every  $P$ . He does not require that  $M$  be super-ordinate to  $S$  and sub-ordinate to  $P$  in the sense Patzig gives to 'super-ordinate' and 'sub-ordinate'. Yet singular terms and categories fail to satisfy axioms (a) and (b) respectively only, Patzig implies, because of these additional requirements. I conclude therefore that Patzig's new explanation of the exclusion of singular terms from Aristotle's theory of syllogism is not satisfactory.

2 Chapter 2 is devoted to a study of Aristotle's concept (or concepts) of necessity. He was well aware of the difference between 'necessarily' in 'men are necessarily animals' and 'necessarily' in 'if  $P$  belongs to every  $M$  and  $M$  belongs to every  $S$ , necessarily  $P$  belongs to every  $S$ ', and he distinguished these as 'absolute' and 'relative' necessity. In a valuable investigation of Aristotle's terminology Patzig shows how careful he usually was to avoid confusion between the two types of necessity.<sup>1</sup> Patzig argues,

<sup>1</sup> Patzig adduces as a convincing proof of Aristotle's careful use of terms the fact that in the modal logic  $\xi\ \delta\acute{\iota}\alpha\kappa\tau\eta\varsigma$  always stands for absolute necessity, never for relative necessity. But see 34a7, 17, 21.

however, that Aristotle failed to understand relative necessity and wrongly treated 'necessarily' in a (categorical) syllogism as attaching to the conclusion (or, better, apodosis) when in fact it marks the necessary *connection* between premisses and conclusion (or, better, protasis and apodosis)<sup>1</sup> It would certainly have been clearer if Aristotle had written 'necessarily if . . . then . . .' instead of 'if . . . then necessarily . . .'; though I am not sure that Patzig's references to *De Int.* c. 9 and *An. Pr.* 30b31-40 suffice to show that Aristotle was really misled by his way of talking Patzig's own account of relative necessity is similar to Łukasiewicz's 'the Aristotelian sign of syllogistic necessity represents a universal quantifier' (Łukasiewicz, p. 11). He next argues that the two sorts of necessity distinguished by Aristotle are really identical, since both represent universal quantification, in one case quantification over individual-variables, in the other case quantification over concept-variables. His argument (pp. 43-45) for construing 'all men are necessarily animals' as saying no more than '(x) if x is a man, x is an animal' is too compressed and rather confused. As requirements for 'all men are necessarily animals' he mentions both that the implication 'if x is a man, x is an animal' must hold for all values of x at all times, and that this should follow from the definitions of the terms 'man' and 'animal'. But if these are two different requirements the treatment of 'all men are necessarily animals' as a universally quantified implication-statement will certainly not do. Professor J. Hintikka has indeed argued that because Aristotle holds that every possibility must be realised at some moment of time the notion of universality is for him identical with that of (absolute) necessity<sup>2</sup>. This suggestion is not without difficulties: for instance, as Hintikka allows, it prevents any distinction being drawn between assertoric and apodictic universal propositions. In any event Patzig does not explicitly make this suggestion, and his own position remains somewhat obscure.

Some points of detail: (a) it is clear that in these pages of the book Patzig's arrow-sign stands for material implication. In other places, where he is symbolising logical theses, it could be standing for strict implication but probably is not, it is probably being used as the hook-sign commonly is in the citing of logical theses. If this is so, the explanation Patzig gives when he introduces the arrow-sign is wrong and misleading: he says (p. 12) that '*MeP* & *SiM* → *SoP*' is to be read as '*SoP* follows from *MeP* & *SiM*'. If, on the other hand, this is what he means by the arrow-sign he certainly ought to use a *different sign*, standing for material implication, in

<sup>1</sup> In what follows I shall sometimes speak, as Patzig does, of premisses and conclusion, although these terms are not strictly appropriate in discussion of the standard Aristotelian syllogism.

<sup>2</sup> J. Hintikka, 'Necessity, Universality, and Time in Aristotle', in *Eripanos Ajatus*, XX, 1957. I owe my knowledge of this paper to the kindness of Mr. Peter Geach.

some parts of the book. (b) The last pages of the chapter contain, as well as a justified complaint about Maier's obscurity, an unjustified blast against Łukasiewicz. Patzig says that Łukasiewicz says nothing about the distinction between relative and absolute necessity, that this neglect is characteristic of his procedure, and that his narrow concentration on certain passages diminishes the value of his book as a commentary—'der es doch sein will'. But Łukasiewicz's first edition was explicitly confined to the non-modal part of Aristotle's syllogistic and so had no need to discuss 'absolute' necessity, while the second edition carefully distinguishes the two sorts of necessity and discusses both (Index s.v. 'necessity'). (c) Patzig in a note on page 50 claims Ross's support for taking  $\xi\zeta$   $\acute{\alpha}\nu\acute{\alpha}\gamma\kappa\eta\varsigma$  in 75a20 to stand for absolute necessity and  $\acute{\alpha}\nu\acute{\alpha}\gamma\kappa\eta$  in 75a23 to stand for relative necessity. But the note quoted from Ross refers to a different sentence (75a25-27). Ross, as is clear from his *Analysis* (p. 528), takes both phrases to refer to absolute necessity; and he is surely right.

3 Chapter 3 investigates Aristotle's distinction between 'perfect' and 'imperfect' syllogisms. Patzig first clears up some old misconceptions. Aristotle does not regard imperfect syllogisms as lacking full validity, but simply as lacking the self-evidence of perfect syllogisms. Nor does he think syllogisms are perfect *because* they are in the first figure; indeed he classifies two first-figure moods, in the modal logic, as imperfect<sup>1</sup>. What formal properties are possessed by all and only the syllogisms Aristotle calls perfect? Is a syllogism with these properties really evident in a way in which no other syllogism is? These are Patzig's questions.

From this point (p. 58) Patzig gives a meaning to the symbols ' $a$ ', ' $e$ ', ' $i$ ', ' $o$ ', which differs from that in traditional expositions but corresponds to Aristotle's standard way of formulating syllogisms. ' $AaB$ ' will mean ' $A$  belongs to every  $B$ '. Thus for *Barbara* we have: if  $PaM$  and  $MaS$ , then  $PaS$ . It now leaps to the eye that only in the first figure are the first and last terms of the protasis also the first and last (i.e. second) terms of the apodosis; and that only in this figure is the so-called middle term, which links the other terms, in fact in the middle, i.e. between  $P$  and  $S$  in the protasis. Only first-figure syllogisms can be translated forthwith into the symbolism of two-place relational logic. Thus  $Ae/aC$  corresponds to the protasis of *Celarent*. The protasis of a second-figure syllogism cannot be written in the form  $Ax/yC$  unless one allows converse relations, writing, for instance, ' $Pe/\bar{a}S$ ' for the

<sup>1</sup> Patzig says that Aristotle gives as an argument for assigning first place to the first figure the fact that 'sämtliche vollkommenen Schlüsse dieser Figur angehören' (p. 53). This is presumably a reference to the end of *An. Pr.* A 4. But what Aristotle says here is not that all perfect syllogisms belong to this figure, but that all syllogisms in this figure are perfect: πάντες οἱ ἐν αὐτῇ συλλογισμοὶ τέλει εἰσι. Aristotle has in mind here of course, only assertoric syllogisms.

protasis of *Cesare*. Now the two relevant features of a first-figure syllogism as normally formulated by Aristotle disappear if *either* the order of the conjuncts in the apodosis is reversed *or* '*A* belongs to *B*' is replaced by '*B* is *A*'; though they re-appear if *both* these changes are made: 'if every *S* is *M* and every *M* is *P*, then every *S* is *P*'. Traditional expositions make only the second change and thus effectively conceal the formal features of first-figure syllogisms which go far to explain Aristotle's regarding them as essentially more evident than any syllogisms in other figures. Aristotle does himself sometimes formulate syllogisms in the '*A* is *B*' rather than the '*B* belongs to *A*' manner. But in the one place in *An. Pr.* A 4-6 where he does this he also changes the order of the conjuncts of the protasis; and in many, though not all, other places the same is true. The ancient commentators behave in the same way, switching premisses where they use the '*A* is *B*' kind of formulation, without however realising that the formal features of first-figure syllogisms which they thereby preserve explain Aristotle's distinction between perfect (or evident) and imperfect syllogisms.

It remains to be seen how this explanation of the perfection of some syllogisms fits Aristotle's modal logic. Some but not all first figure modal syllogisms are said to be perfect. Why? Patzig's discussion of this is too complicated to be summarised adequately. He follows Becker's view that for Aristotle the modal factor belongs not to a proposition but to a term. Thus examples of first-figure modal syllogisms are (a)  $NAaB \ \& \ BaC \rightarrow NAaC$  (30a17), (b)  $NAaB \ \& \ NBaC \rightarrow NAaC$  (29b36), (c)  $AaB \ \& \ MBaC \rightarrow MAaC$  (34a34). Only in (a) is the second term of the first conjunct identical with the first term of the second conjunct; only in (a) and (b) is the first term of the first conjunct identical with the first term of the apodosis. Both (a) and (b) are called perfect, (c) is not. Patzig seeks to explain Aristotle's decisions as to which first-figure modal syllogisms are to count as perfect, though he thinks that Aristotle in fact allows as perfect some syllogisms which, since they require the use of supplementary logical operations to give them the desirable formal properties of first-figure assertoric syllogisms, ought not to be counted as perfect, i.e. immediately evident. I will make only three points about this valuable section of Patzig's book. (1) In connection with the syllogism ' $MAaB \ \& \ MBaC \rightarrow MAaC$ ' (32b38-33a1) Aristotle introduces ' $MAaMB$ ' as an interpretation of τὸ *A* παντὶ τῷ *B* ἐνδέχεται ὑπάρχειν. He does this, Patzig suggests, in order to justify re-writing the above syllogism as ' $MAaMB \ \& \ MBaC \rightarrow MAaC$ '; this gets over the 'difficulty' that otherwise the syllogism will not have an identical middle term and will not have all the happy features of a first-figure assertoric syllogism. However, since there are, according to Aristotle and Patzig, some first-figure modal syllogisms which lack an identical middle term and are imperfect, Aristotle could perfectly well have classified the above syllogism as imperfect.

Why did there appear to be a 'difficulty' which had to be overcome (by a rather dubious device)?<sup>2</sup> (2) With reference to the two imperfect first figure syllogisms ' $AaB$  &  $MBaC \rightarrow M'AaC$ ' and ' $NAaB$  &  $MBaC \rightarrow M'AaC$ ', Patzig says that Aristotle's calling them imperfect shows that it is for him a sufficient condition of a syllogism's being imperfect that the predicate of its conclusion should fail to be identical with the predicate of its first premiss. The two syllogisms in question, however, lack an identical middle term, and for them Aristotle offers no device (like turning ' $MAaB$ ' into ' $MAaMB$ ') to correct this. So his classification of them as imperfect need depend only on their lack of an identical middle term. (3) There is a certain incoherence between Patzig's treatment of absolute necessity in Chapter 2 and his adoption of Becker's view of modal factors in Chapter 3. Chapter 2 construes 'necessarily  $A$  is  $B$ ' as ' $(x) x$  is  $A \rightarrow x$  is  $B$ ', Chapter 3 construes it as ' $(x) x$  is  $A \rightarrow x$  is necessarily- $B$ '.

4. In Chapter 4 Patzig studies Aristotle's definitions of the different figures of syllogism and of such terms as 'middle', 'major', etc. His main purpose is to explain why Aristotle recognises only three figures, though acknowledging as valid the moods of the traditional fourth figure. Łukasiewicz treats Aristotle's omission of these moods from his systematic classification of syllogisms as an oversight, and he supposes, with Bochenski, that *An.Pr.* A.7 and B.1, where these moods are mentioned, were composed later than the main exposition and never incorporated into it. Patzig studies the relevant texts very closely and explains the omission of the fourth figure as a consequence of Aristotle's way of defining figures and terms. In *An.Pr.* A.4-6 Aristotle characterises his three figures separately, giving new explanations in each case of the expressions 'middle term', 'extreme terms', etc. Patzig analyses these passages skilfully.<sup>1</sup> He next considers Aristotle's attempts in *An.Pr.* A.23 and 32 to give *general* definitions of 'middle', 'major', etc. He then develops his own solution to the problem of the missing fourth figure, and ends the chapter with a critical survey of the views of some ancient and modern commentators.

The discussion in this chapter is full, close and rather complicated. There are many interlocking points and many minor topics which cannot be taken up here. The essential point is perhaps this. If one considers two premisses containing three terms in all, and neglects the order of the premisses, the terms fall into one of only three patterns. The shared term may be predicated (affirmatively or negatively) of both the other terms; it may have them both predicated of itself, it may have one predicated of itself and be predicated of the other. Since the order of premisses is being neglected, there is no difference between ' $AxM$  &  $MxB$ ' and ' $MxB$  &  $AxM$ '.

<sup>1</sup> But he follows Łukasiewicz in making one or two criticisms of Aristotle which Austin has shown to be ill-considered (MIND, 1952, pp. 398 f.).

Both exemplify the third pattern. The natural way of writing such premisses is ' $AxM$  &  $MxB$ '. Now Aristotle combines this trichotomy with a convention (derived from his idea of perfection or self-evidence) that the first-mentioned (*i.e.* predicate) term of the conclusion shall be the first-mentioned of the two 'extreme' terms in the premisses. But this makes it impossible for him to recognise the fourth figure, since it is indistinguishable from the first according to the (perfectly reasonable) trichotomous classification of pairs of premisses, and cannot (because of the convention) be distinguished by reference to the order of terms in the conclusion. He therefore implicitly handles moods of the traditional fourth figure as indirect moods of the first figure. Theophrastus, of course, explicitly puts them into the first figure,—and ought, by the same token, to give the indirect moods of the second and third figures places in those figures. 'Die vierte Figur fehlt bei Aristoteles, weil sie im Rahmen des von Aristoteles in A 4-6 entwickelten Systems nicht definiert werden kann'. This general explanation (buttressed by much detailed argument) does not acquit Aristotle of error or confusion. But it does show how various of his steps and assumptions worked together to leave him in trouble, and it is much more illuminating than a mere appeal to oversight or failure to revise.

5 Chapter 5 asks whether Aristotle's syllogistic is a deductive axiomatised system. Patzig concludes that it is, and that conversion, *reductio ad impossibile* and ekthesis are methods of proving the (non-axiomatic) theses of the system. Aristotle however does not make this clear because he is wedded to the idea that every proof must be in syllogistic form, whereas in fact the proof of a syllogism is not itself a syllogism. Aristotle fails to lay bare the real form of a proof of a syllogism and so fails to found propositional logic. He uses misleading descriptions, speaking as though in direct reduction one syllogism is *turned into* another. His theory lags behind his practice. Patzig's discussion of all this is clear and full.<sup>1</sup> The next section of the chapter analyses carefully the proofs by conversion and finds them to be correct, given the conversion-rules of *An Pr* A 2 and certain true (though by Aristotle unformulated) theses of propositional logic. Proofs through *reductio ad impossibile* give more trouble. Traditional descriptions of such proofs speak of premisses as assumed to be, or given as, true, and they speak as though the purpose of such a proof is to show the conclusion of the reduced syllogism to be true. The required conclusion, however, is really that the reduced syllogism is true, and for the proof of this nothing has to be assumed as to the truth of any of the ingredient propositions. Is the traditional description a fair account of the proofs Aristotle gives? Łukasiewicz assumes that it is and so very properly criticises Aristotle: 'the proof given by Aristotle is neither sufficient nor a proof by *reductio ad impossibile* it can easily be seen that

<sup>1</sup> There is an unfortunate slip on page 139, where '*A, B und D, E*', appears twice instead of '*D, E und A, B*'.

thus (i.e. Łukasiewicz's) genuine proof of the mood Baroco by *reductio ad impossibile* is quite different from that given by Aristotle (pp. 54-56). Patzig argues that Aristotle can be defended against this criticism and that he was well aware of (though not sufficiently explicit about) the difference between proving a syllogism by *reductio ad impossibile* and proving an ordinary proposition by a method related to, but different from, the method of proving a syllogism. Patzig's argument is not easy to summarise, but the following are some of the main points. (a) Aristotle knows that *Bocardo* and *Baroco* cannot be 'perfected' *δεικτικῶς* but only *διὰ τοῦ ἀδυνάτου*. Yet he says twice (45a26, 62b38) that whatever can be proved by a deiktic syllogism can also be proved (with the same terms) by a syllogism *διὰ τοῦ ἀδυνάτου*, and *vice versa*. This and other evidence shows that he distinguishes between a *sylogismus per impossibile* and a *per impossibile* proof of a syllogism. (b) A *sylogismus per impossibile* is indeed a way, alternative to the deiktic syllogism, of proving a conclusion from certain agreed premisses. 'From the premisses *AeB* and *BiC* I can prove by a deiktic syllogism (*Ferio*) the proposition *AoC*. I can prove the same proposition from the same premisses *per impossibile* if I combine its contradictory (*AaC*) with *AeB* and get (by *Cesare*) *BeC*, and then argue from this and the agreed premisses *BiC* and *AeB* to *not-AaC* and so to *AoC*' (p. 160). A *sylogismus per impossibile* is addressed to one who admits *p* and *q* and is then made to admit *r* by an appeal to '*p & not-r* → *not-q*'. (c) A *per impossibile* proof of a syllogism, on the other hand, is directed to one who admits '*p & not-r* → *not-q*' and is then made to admit '*p & q* → *r*'. Such proofs are closely related to Aristotle's conversions of syllogisms in *An. Pr.* B 8-10: *reductio per impossibile* derives *Baroco* from *Barbara*, conversion converts *Baroco* into *Barbara*. (d) Aristotle's *per impossibile* proofs of syllogisms are not carefully worded, and he does not take sufficient care to exclude the traditional interpretation. But his words can be taken in a way which makes them a correct account of such proofs of syllogisms, and since he is otherwise involved in an evident contradiction—(a) above—it is right to give him the benefit of any doubt at this point.

Patzig's discussion of these topics and passages is certainly illuminating. On the crucial question how much Aristotle saw, the following points deserve consideration. First, while from one point of view there is the utmost difference between a *sylogismus per impossibile* and a *reductio per impossibile*, they are also very closely related. They are as close as are the two theses '*p & not-r* → *not-q*' & '*p & q* → *r*' and '*(p & not-r* → *not-q*) → (*p & q* → *r*)'. It must be a very delicate question whether to emphasise the difference or the similarity in coming to a view about what Aristotle probably saw. Secondly, it is not easy to dissociate *An. Pr.* B.8-10 from B.11 ff. in the way they must be if Aristotle is supposed to have seen the difference between *per impossibile* proof and the



converse operation of converting syllogisms on the one hand, and *sylogismus per impossibile* on the other. Nor is it easy to distinguish the wording of the A.5, 6 proofs from that of B 11 ff. Thirdly, the words Aristotle uses in his *per impossibile* proofs of syllogisms suggest not merely inexplicitness but error as to the true nature of the operation. Such phrases as *ὑπέκειντο δὲ τῷ μὴ ὑπάρχειν* (2761) and *τεθέντος τοῦ ψεύδους* (29a35) tell against Patzig's claim on Aristotle's behalf, and they are not explained away by him. I conclude that his discussion is instructive and provocative but not conclusive.

Patzig's analysis of ekthesis as a type of proof of syllogisms is similar to Łukasiewicz's (*Aristotle's Syllogistic*, pp. 59-66).<sup>1</sup> He claims that this interpretation is strongly confirmed by the passage in the modal logic (which Łukasiewicz refers to but does not analyse) where Aristotle gives proofs by ekthesis of *Baroco* and *Bocardo* with necessary premisses and conclusion (30a6-18). Patzig's account of this passage differs from Ross's (*Analytica*, p. 317) in bringing out the full structure of the argument more clearly. More controversial is his claim that the two logical laws on which ekthesis depends, though not expressly stated by Aristotle where he gives proofs by ekthesis, are nevertheless laid down by him elsewhere. The laws are ' $A \cdot B \leftrightarrow (\exists C) (AaC \ \& \ BaC)$ ' and ' $A \cdot O \leftrightarrow (\exists C) (AeC \ \& \ BaC)$ '. Patzig cites *An. Pr.* A.28, 43b43-44a2 and 44a9-11, and says: 'Hier ist deutlich ausgesprochen, das die Existenz eines Mittelsbegriffs *C*, der den beschriebenen Anforderungen genügt, notwendige (*βλεπτόν*, 43b40) und hinreichende (*ἀνάγκη*; 43b42, 44a1-11) Bedingung der Gültigkeit der Sätze  $A \cdot B$  und  $A \cdot O$  ist, und nichts anderes behaupten ja die eben aufgestellten Äquivalenzen' (p. 172). Patzig's use of this chapter in connection with the exclusion of singular terms from syllogisms has already been criticised. Here too the chapter seems to be misunderstood. *βλεπτόν* refers to what we must look for and find if we are to be able to *prove* some proposition. It does not refer to what must be the case if the proposition is to be true. So it does not imply ' $A \cdot B \rightarrow (\exists C) (AaC \ \& \ BaC)$ '. The fact that for any proposition  $A \cdot B$  one can 'construct' a term *C* such that  $AaC$  and  $BaC$ , is certainly not anything Aristotle has in mind in *An. Pr.* A.28, where he is talking about the tackling of real problems of proof the solution to which requires us to be equipped with all sorts of truths about the relevant subject-matter. He is not giving a simple procedure for constructing a formally valid proof of any proposition whatever of the form  $A \cdot B$  ( $AaB$ , etc.), he is giving guidance on how to survey available truths on a given subject-matter if one hopes to demonstrate some definite proposition of the form  $A \cdot B$  ( $AaB$ , etc.).

The last section of Chapter 5 discusses Aristotle's proofs that certain combinations of premisses yield no conclusion. Where he

<sup>1</sup> A simpler alternative interpretation has been suggested by Ivo Thomas in *Dominican Studies*, 1950, pp. 183 f.

can, Aristotle uses this kind of argument - from  $AaB$  and  $BeC$  nothing follows (i.e. neither  $AaC$  nor  $AeC$  nor  $AiC$  nor  $AoC$ ): for one can find two trios of terms which satisfy respectively ' $AaB$   $BeC$  &  $AaC$ ' and ' $AaB$  &  $BeC$  &  $AeC$ ' (e.g. animal, man, ox: animal, man, stone). Where difficulty arises with this method Aristotle uses a proof *ἐκ τοῦ ἀδιόριστου* which deduces the inconclusiveness of one pair of propositions from the already established inconclusiveness of a different pair. Łukasiewicz criticises Aristotle for using the method of concrete terms, but Patzig rightly defends it. To deny that a certain implication holds for all values of  $A$ ,  $B$  and  $C$  is to say that there are values for which it does not hold; and this can be shown only by the production of some such values. Łukasiewicz deduces the rejection of all the traditional invalid moods from two 'axioms of rejection'. But Patzig claims that these are not self-evident like perfect syllogisms but can be made acceptable only by the procedure of producing appropriate concrete terms, which procedure is therefore not only perfectly satisfactory in itself but also in a way prior to the purely axiomatic procedure recommended by Łukasiewicz. Among the good things in this section should be mentioned Patzig's convincing analysis of Aristotle's invalid argument at 26a39-b10, which is an improvement on Ross's account (*Analytics*, p. 304).

The book ends with a bibliography and Indices of passages Greek terms, names and topics.

This is an instructive and stimulating work which will be studied with pleasure and profit for many years to come.

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*The Philosophy of C. D. Broad.* The Library of Living Philosophers edited by PAUL ARTHUR SCHILPP. New York. Tudor Publishing Company (U.K. Cambridge University Press), 1960. Pp. xii + 866. 110s.

'A philosopher is an animal in which the scientist vanishes into the logician'. Thus Wisdom concludes one of his beguiling little bestiaries illuminating the Cambridge scene (Moore's Technique, 1942). Broad, you may recall, was the specious animal who cut scientific capers across the counterpoint of Moore's logic-chopping. Now we have an opportunity to study in relatively compendious form the interplay between theory and practice which occasioned this rather singular interim reputation, and to decide whether the hunt at hocus-pocus is valid.

In Broad's writings on method critical and speculative philosophy appear as separate 'branches' which are differentiated from each other in terms of a further distinction between the activities of analysis, synopsis, and synthesis. Critical philosophy consists primarily of the analysis of concepts and can aspire to certainty.

Speculative philosophy consists primarily of the other two activities and is doomed to doubt. As amplified in detail garnered from various sources by Körner ('Broad on Philosophical Method'), Broad's main distinction, however, remains rather indeterminate. Apparently Broad finds all three activities in both branches (see, e.g. p. 98). Yet he believes the two to be independent. speculation, indeed, should lean upon critical philosophy, but not vice versa. Since this 'should' is presumably logical rather than moral, how can speculation be independent? Botched philosophy is not a separate branch.

The distinction between analysis, synopsis and synthesis is left equally obscure. Synopsis, perhaps is unmistakable. Broad and the critics who retrace his footsteps offer excellent examples in this volume; and from these I select a few. Hanson in 'Broad and the Laws of Dynamics' presents an impressive catalogue of different uses to which these laws may be put so that we can determine their logical status. Ducasse ('Broad on the Relevance of Psychological Research to Philosophy') and Flew ('Broad on Supernormal Precognition') extend our attention to paranormal phenomena to discover what modifications, if any, need be made to the conceptions of cognition, or time, or causality, or action, which so far have stood more normal phenomena in good stead. Kneale ('Broad on Mental Events and Epiphenomenalism') and Ducasse examine different manifestations of mind-body relationship in order to decide between or against interactionism and epiphenomenalism. Price ('The Nature and Status of Sense-Data in Broad's Epistemology') surveys various forms of consciousness to decide whether in all cases an act-object or an internal accusative analysis is appropriate. Marc-Wogau ('On C. D. Broad's Theory of Senses') gives the phenomena of illusion, etc., a further once-over to decide whether a semi-naïve-realist interpretation of senses will do. Hedenius ('Broad's Treatment of Determinism and Free Will') examines the interconnection between the notions of obligation, action, moral responsibility, causation, and freedom, or, if you like, the fields within which those concepts are respectively applied, to decide whether or not there is any incompatibility between obligation or moral responsibility on the one hand, and acting freely on the other. It will be noted from this list that in philosophy as in science the mutual relevance of the synoptically related fields can become apparent only in terms of some specific problem or call to synthesis.

Synopsis tends, in the later parlance of Ryle, to instigate litigation between teams of concepts as to their respective spheres of application. Synthesis, on the other hand, resolves these conflicts by giving an analysis. Analysis and synthesis therefore, can not be distinguished as two co-ordinate activities. for in so far as analysis is contrastable with synthesis it is not an activity at all but merely the end product of synthesis. Synthesis consists in arranging general category statements in different logical patterns until logical

friction or disconnectedness is reduced to a minimum as when Broad affirms (pp 781-4) or Flew denies (pp. 423 f.) that since by definition veridical precognising is not just lucky guessing it could only be inferential based upon some direct or indirect causal connection between foreseeing and foreseen. An analysis is such an arrangement

The contrary assumption may have been acquired by Broad from Moore, who seems to have looked upon analysis as an unaccountably difficult process of bringing complex concepts before the attention and there by some logical chemistry discovering the simpler parts in their interrelationships. Since there is no obvious place for hypothesis in this account, Broad seems to treat it as accurate merely for one part of philosophical endeavour, whereas in fact it accurately describes no part. Philosophy cannot be partitioned on these lines, and except for the *ad hominem* no philosophical conclusion can aspire to certainty. One is tempted to suppose that Wisdom had a clearer inkling that to distinguish synthesis from analysis as between two co-ordinate activities is to remove the one positive interest which differentiates philosophical synthesis from the forming of scientific hypotheses, viz. its concern with logical connections between different category statements, but that he attributed this anomaly to philosophy as such instead of to a faulty conception of analysis. Just as Kierkegaard and Bergson took their norm for reason respectively from Hegel and Descartes in order to replace reason by paradox, mixed metaphor, and nonrational intuition, so Wisdom seems to have taken his norm for analysis from Moore in order to replace philosophical insight by a decorative illumination more in keeping with the Book of Kells. Except in perfectly hospitable senses of 'logic' and 'geometry', giving an analysis is no more like doing logic than cartography is like doing geometry.

In justice to Wisdom one should perhaps note that his verdict was based at least partly on the fact that many of the philosophical hypotheses entertained by Broad tended to challenge or confirm statements which are underwritten by common-sense. Yet according to Wisdom, when it came to the test Broad was unwilling to incur Moore's histrionic horror by questioning paradigmatic truths like 'Here is a hand' or 'I hear a bell'. Instead, he challenges a particular analysis of these specimens and hence, according to Wisdom, relapses into logic or *doing* analysis.

I have already dealt with the alleged relapse. As for the rest of the criticism, Broad may have been less ambivalent in his attitude towards common-sense than Wisdom allows. At least three applications of common-sense seem relevant here.

- (1) The orthodox application patented by Moore in which common-sense supplies us with certain particular facts and leaves to philosophy the task of supplying an analysis of those facts in terms of technical categories such as sense-data, natural and non-natural qualities, etc.

- (2) A heterodox application in which common-sense supplies analyses in terms of such categories.
- (3) The 'cult of ordinary language', which if we assume that ordinary language is the expression of common-sense, brings the latter into direct conflict with the sort of analysis permitted by (1) and (2).

There certainly are occasions when Broad rejects all three applications.

He has rejected the orthodox application in deflationary style as when in his *Inaugural Lecture* of 1934 he argued for moral scepticism in order to make sense of voluntary action, and in inflationary style as when in his reply (pp. 743 ff.) to Minnie, 'Broad's Views about Time' he endows time with a second dimension mainly in order to make sense of our ordinary use of temporal predicates. In general I subscribe to the view that without some sort of knavery for whom our concepts philosophy would grow Buchenianite and soft, though of course like Graham Greene's hell the knavery may in fact be empty. However, I hold no brief for an inflation of the spatio-temporal world based purely upon logical considerations. Words are devised for the purpose of coping with our experience, and if they did not cope that would not in itself point to the existence of something hitherto unsuspected. Nor do I hold with his particular essay on deflation: though on the other hand I accept his rejection (p. 823) of Hederius's reconciliation between moral responsibility and freedom in terms of a Hart-like analysis of causal terms.

On other occasions, noted with concern by Marc-Wogan (pp. 495 f.) and Tolson ('Broad's Views on the Nature and Existence of External Objects', pp. 514-515), he has turned common-sense to heterodox use before turning inwards. He has allowed common-sense to suggest that the temporal relations between sense in the same sense-history are dyadic, and elsewhere to suggest a dualist ontology. He has then invested these analyses with some degree of priority, but treats them as less than sacrosanct. In this respect—*if* one can describe Aristotle's method as such—he remains adamant: and here once more I have no criticism of the principle as distinct from its applications. He concedes in his reply (pp. 834-835) that the philosophical sense of common-sense is not apparent to the ordinary man, but insists that it can be elicited without undue difficulty.

The third application of common-sense became something like neo-orthodoxy in the years that intervene between the completion of Broad's major philosophical work and 1954 when most of the critical essays in this volume had been sent in. Probably it had passed its peak by that date at least in its more extravagant forms. On Kneale's testimony (p. 445), Broad's use of 'experience' and 'event' had come under fire as not generous in the context of the philosophy of mind. Broad accepts (p. 781) Kneale's attack on the cult, and with little addition.

It is, of course, perfectly legitimate to appeal to ordinary language:

for the purpose of exposing confusion in philosophical usage. Thus Mundle shows (pp. 361-362) how Broad applies the word 'event' in the context of his philosophy of time in a way which confuses among other things achievement, task, and process words. The method only becomes objectionable where departure from English idiom is *ipso facto* regarded as bad logic. Broad's theory of Absolute Becoming, in which he correctly (see my views in *MIND*, July 1957) traces the distinction between past, present and future to an ontological rather than a linguistic (Mundle) or a subjective (Ducasse) distinction, comes in for criticism of this sort. Thus Mundle thinks (p. 360) that Smart's insistence in 'The River of Time' (*MIND*, October 1949) that '*things* change, *events* happen' is sufficient to dispose of the paradox alleging that events change in the course of time with respect to the characteristics of being past, present and future. To see that it is not sufficient it is sufficient to observe that in certain rural districts in Scotland the rustics deprive their dialect of a generically useful word by refusing to classify wheat as anything but wheat and confining 'corn' solely to oats. Likewise Ducasse attaches undue weight (p. 394) to Smart's dictum 'We can say that the new republic came into existence then, but we cannot say that the inauguration came into existence'. This proscribed locution so far as it goes could, on the contrary, be as legitimate as less stilted expressions such as 'There are (have been/will be) floods up North'.

My conclusion is (a) that Broad flouted commonsense in its third application more or less consistently, if unpremeditatedly, throughout his career, though he did occasionally stigmatise jargon as 'barbarous' before this became a philosophical cliché, (b) that his rejection of the first and second applications constitute two distinct kinds of move occasioned by different philosophical problems. In rejecting these applications, therefore, he is not in general moving between opposing poles of indecision over the same problem—though of course there may be occasions where he is undecided between two of those moves. This goat is not an animal in which cow, sheep and antelope vanish into one another, as Wisdom argues in disconcerting feed-back from his conclusion to its illustration. Like any other goat I know of, it is like all these animals in different or overlapping respects.

Admittedly Broad's practice would have been less vulnerable to misunderstanding had his theory of method been more coherent. Korner proposes a distinction (pp. 100 f.) between two types of analysis which seems to fulfil the intention of Broad's tripartite division more effectively. One of the two exhibits the rules whereby the use of concept signs are in fact governed. The other presupposes the first and seeks to replace these rules in so far as they are considered to be logically defective in some way. Both involve synopsis and synthesis. While this seems a more apposite account of Broad's methodological ethos than his own, I have a number of reservations among which two seem particularly relevant here.

(1) According to Körner 'exhibition-analysis results in empirical, replacement-analysis in logical propositions' (p. 107). Surely this depends upon whether the analysis is given in the formal or material mode of speech. If in the material mode (*e.g.* 'A material object is a family of sense-data') both types of proposition are true or false by virtue of certain rules for concept signs belonging to the formal mode. Hence both are logically valid or invalid given these rules. If they are in the formal mode (*e.g.* 'Words denoting sense-data related in certain ways are/should be substitutable in meaning for material-object words') the exhibition-analysis is certainly empirical, but on the other hand the replacement-analysis is not a logical proposition. Analyses of both kinds are offered for the purpose of eliminating apparent or real logical conflicts deriving from our use of words in the same way as scientific hypotheses are offered for the purpose of eliminating apparent causal anomalies. In neither type of analysis is there any guarantee that any given solution will not raise more intractable problems than it solves, or that, even if it does the job well, other solutions wouldn't work equally well or better. Hence while replacement-rules are designed to eliminate what is *a priori* invalid, in the formal mode their own validity need not be *a priori* and of course cannot be *a posteriori*.

(2) Contrary to Broad both Körner (*op. cit.*) and Nelson ('Some Ontological Presuppositions in Broad's Philosophy', p. 74) believe that critical philosophy must lean to some extent upon speculation. For Körner speculative philosophy becomes legitimate, and is indispensable, where it provides regulative principles for choosing between alternative replacement-analyses. It seems to me, however, that in substituting his distinction between two sorts of analysis in place of Broad's trinity of analysis, synopsis, and synthesis he has in fact removed whatever basis Broad's distinction between critical and speculative philosophy may appear to have had. Perhaps the vagueness of Körner's examples at this point may have misled me, but why should the generality of a policy statement 'to the effect that such concepts . . . as have a certain characteristic, say a certain type of inexactness, should be rejected, and that they should, where possible, be replaced by concepts . . . free from this defect, provided that the replacing and the replaced concepts stood in a certain relation to each other' (p. 110) prevent it from being as much a replacement-analysis as the particular replacement-analyses which it governs? Policies governing choices are just as much objects of choice as the choices which they govern. I suggest that Broad and others mistake in speculation and criticism a distinction between philosophers who take the initiative and those who react critically for a distinction between two branches of philosophy.

Broad was the least gimmick-ridden of all philosophers. To dwell so long on the subject of technique may therefore seem perverse, particularly since the bulk of this volume is taken up by many more interesting matters upon which I must now remain silent. It

is difficult, however, to give an accurate impression of his far-ranging and closely-meshed interests or aims apart from the method which served them. His aspiration was, to quote von Wright slightly out of context ('Broad on Induction and Probability', p 327) to make 'the intricate connections in the conceptual groundplan of nature more perspicuous'. His particular skill lay in detecting points of crisis in our understanding of this plan. His solutions tended at times to be despairingly Dracomian. At other times he contented himself merely with recording opposing inclinations (see Frankena's inquisitorial 'Broad's Analysis of Ethical Terms' and the reply on p 813). In general his syntheses quail before the formidable sweep of his synopsis, but, to quote von Wright out of context once more (p 328), 'to consider various alternatives... is to deepen our insight into the conceptual network of thinking about nature, and is therefore already a major task of a Natural Philosophy or Metaphysics of Nature'. To seek clarity mainly through this sort of indecision may, however, lead to that slackening in philosophical enterprise which on his own submission beset him in his prime. One might perhaps trace to this cause the factually inept quip (pp 811-812) which moves him as late as 1955 to identify Wittgenstein with the Vienna Circle. A legitimate lack of sympathy with his younger colleagues does not mitigate this degree of misrepresentation, though possibly the joke is of very early vintage.

Of the twenty-one critical essays in the volume two are egregiously indigestible. That by Turnbull ('Empirical and A Priori Elements in Broad's Theory of Knowledge') requires and, I would guess merits mastery of his strangulating special vocabulary for the sake of such fundamentally important but neglected topics as the processes of abstraction which he treats with suggestive minuteness. In Browning's 'Broad's Theory of Emotions', on the other hand, I can only see an ambling sprawl. The contributions of Nelson and von Wright, Mundle and Ducasse, Ducasse and Flew, and Broad's acute rejoinders respectively on the subjects of induction, etc., time, and supernormal precognition are particularly nutritive. Within these topics Broad's enquiries stand least in the shifting shade of more recent developments and probably constitute his most original gift to philosophy.

Finally I must pay tribute to Broad's autobiography as a down-to-earth reckoning of sensitive detachment and humane charm so far unmatched in this series.

I have noticed obvious misprints or slips on page 96, line 23, page 487, line 22, page 770, line 21, and page 778 line 38. In the index 'Moore, G. E., 9' refers apparently to Broad's Uncle George, and 'Wisdom, J., 121' to wisdom.



## IX.—NEW BOOKS

*Aesthetics and Philosophy of Art Criticism.* By JEROME STOLNITZ  
Boston: Houghton Mifflin Co. Pp. viii + 510. \$6.95.

THIS is an enterprising and successfully executed text-book in aesthetics. It is designed to introduce students to a philosophical, i.e. critical, examination of the major beliefs about aesthetic matters. With this end in view, the author has presented these beliefs in their classical formulations as well as in their percolated variants voiced by contemporary elementary students of the arts. He has also raised certain questions about these beliefs, especially about some of their key concepts and their empirical truth. Thus, there are careful statements of the traditional, established major aesthetic problems, among them, of the nature of artistic creation, fine art, the aesthetic experience, the value of art, the criticism of art, art and truth, and art and morality. In each case, the statement of the problem is accompanied by full, probing expositions of its ablest recognized dissonants, so that the student is given both a digest of and a set of questions about what certain philosophers, critics, or artists have said on a certain aesthetic issue and its solution.

Mr. Stolnitz begins his critical inquiry with a definition of the "aesthetic attitude", which he takes to be basic in the whole of aesthetics. It is "... disinterested and sympathetic attention to and contemplation of any object of awareness whatever, for its own sake alone" (p. 35). This attitude, he says, is basic because it explains the aesthetic experience (as "... the total experience had while this attitude is being taken") the aesthetic object (as "... the object toward which this attitude is adopted"), and aesthetic value (as "... the value of this experience or of its object", p. 42).

Fine art is a sub-class of aesthetic objects. Here Mr. Stolnitz details especially three historically important theories about its nature: that fine art is essentially imitation, that it is essentially form, and that it is essentially expression. He then points out the varying degrees and kinds of inadequacy of these essence theories, e.g. that they neglect some features of art and exaggerate others; or that they are vague, circular, or narrow; or that they prescribe rather than describe the essence of art. The inadequacies of the theories, he says, are rooted in the vast complexity of art.

Yet when he turns to one species of fine art, comedy, and the various theories of it, he suggests that their conflict as well as their inadequacies rest on more than the vast diversity of comedies in literature. He writes: "These theories conceive of comedy in such different ways that, if we take them all together, 'comedy' will no longer have a single, unitary meaning. There will be no property or characteristic common to all instances of comedy. . . . Can we retain 'comedy' as a meaningful term and still respect the differences between the many kinds of comedy?" (p. 298).

In answering his latter question in the affirmative, Mr. Stolnitz suggests that the failure of the theories of comedy is more than the empirical one of not covering all the facts, but a logical one of misconceiving the concept altogether. "Comedy" is and can be employed intelligibly in our talk about comedies without assuming or asserting an essence of the comic; (Wittgenstein's) family resemblances suffice as a condition of

such talk. Thus, theories of comedies commit the logical mistake of assuming that the concept of comedy must have a corresponding essence in order to guarantee intelligible criticism of the comic in literature.

Mr. Stolnitz may be right about his critique of theories of comedy. But what is surprising is his refusal to apply his analysis to similar concepts, especially the parent one of art itself. Here he remains satisfied with a mere recognition that complexity in art makes a univocal theory difficult along with a consequent recommendation to his student-readers that they make up their minds about which theory or theories hold for them. Now, it seems to me that his "critical introduction" requires more than this philosophical dangle; *i.e.*, he should have reconciled his conclusions on the concepts of comedy and art, especially for those student-readers of his who wish to pursue his critical approach *all* the way.

This wavering on the fundamental roots of failure of aesthetic theories is the only defect of an otherwise fine introductory book to aesthetics. Among its other merits are lists of questions and selected bibliographies at the end of each chapter and some related reproductions of works of art at the end of the book.

MORRIS WEITZ

*French Free-thought from Gassendi to Voltaire.* By J. S. SPINK.  
University of London: The Athlone Press, 1960. Pp. ix + 345.  
50s

THE word *libertin*, freethinker, was applied in the middle of the sixteenth century to members of a Protestant sect in the Low Countries whose central belief was that a divine spirit pervades and is the cause of all things, so that all that is is good: and a belief of this sort is found frequently associated with freethought throughout the period with which Professor Spink is dealing. During his period the word *libertin* meant primarily a person who called in question ordinary beliefs, especially those relating to the Christian religion. But this is not sufficient to enable us to decide in all cases whether a particular thinker is to be called a *libertin* or not: was Gassendi for example a freethinker? He was never seriously accused of it. Or Descartes? Many cases are quite clear: writers who expounded Deism, or who denied the existence of God, or who attacked all religions as fictitious, or who criticised the authenticity of biblical texts, or who maintained that man has not an immortal soul, would naturally be described as freethinkers, and their writings could not be printed in France, though many of them circulated in manuscript copies underground. Again, a writer who, while not explicitly asserting any of these things, expounded doctrines from which a reader could be stimulated to infer some of them, was liable to be accused of freethinking; *e.g.* the view that animals are capable of sensation and of a low degree of reasoning, and yet have a soul which is purely material, could be branded as dangerous, because it suggested the further conclusion that there is no need to attribute a different kind of soul to man in order to explain his capacity for thinking. On the other hand, scepticism in regard to ordinary beliefs was quite compatible with a submissive acceptance of revealed truths of religion, and atomism could be and often was coupled with a full acceptance of Christianity. In between these extremes there are a few writers whose doctrines are described by Professor Spink.

where I found it difficult to see what caused them to be included in the list of freethinkers

The history of freethought during the 120 years between Vanini and Voltaire, as described by Professor Spink, follows two main lines, one linked with Gassendi and Epicurus, naturalistic and empiricist in tendency, the other linked with Descartes and Spinoza, and rationalist. The naturalists of the early part of the century found it congenial to suppose a kind of sensitivity diffused throughout the whole of nature, and there was therefore no difficulty in understanding how sentience and thought could arise from a concourse of atoms, and no difficulty in seeing a kind of purposiveness in the way things behaved. It was not until the latter part of the century, when the ideas of Descartes were widespread, that matter and sentience were sharply opposed.

One of the most interesting parts of Professor Spink's book is his account of the manuscripts which circulated underground in France during the first half of the eighteenth century, in spite of vigorous police action. These fall into two groups, one dealing with the history of religions, especially Christianity, mostly from a deistic point of view, the other containing essays which are atheistic, deterministic and materialistic. As Professor Spink says, freethinkers in general were not aggressive nor dogmatic, but critical of received ideas, well abreast of new knowledge in science, in textual studies, in scholarship of all kinds, and the effect of their influence was to keep men's minds open and flexible. He has been studying these manuscripts for many years, and he gives full references to the French libraries in which they are to be found.

His expositions are extremely clear, and his book, while full of interesting detail and anecdote concerning persons, books, manuscripts, social reactions to ideas, and so on, never seems overloaded. One can return to it many times, with increasing pleasure.

L. J. RUSSELL

*The Philosophy of Mathematics.* By STEPHAN KÖRNER. Hutchinson, 1960. Pp 198 10s 6d

THOUGH modestly described as an introductory essay, this book is dense with historical, mathematical, philosophical and critical material. It is tough going, as anything worthwhile on the concepts of mathematics has to be. The logicist, formalist and intuitionist movements are traced from their origins, brought up to date and criticised. A conceptual analysis of applied mathematics is developed on lines which derive from Körner's *Conceptual Thinking*. All of this is packed into less than 200 heavyweight pages.

Körner's message is that the philosophy of mathematics is both directive and derivative. For although the philosopher is concerned with the workings of extant mathematical concepts he can also recommend new lines of development of these concepts and thus on philosophical as opposed to mathematical grounds. His job is to do both of these things. Körner offers his own account of the application of pure mathematics as a sample of description and the logicist, formalist and intuitionist views as recommendations. He is not out to produce further recommendations but only to display the features of these three approaches to 'foundations'. In particular, he wants to show how the philosophical and the mathematical aspects of these views are related. For while the implementation of a

recommended programme is mathematical, its defence is philosophical. Fortunately he does not keep to this policy of dissection and display. He argues as well.

It is never shown that recommendation of a certain line of development in mathematics can be backed by philosophical as opposed to mathematical reasons. It is never shown that regulative directives are given on independent philosophical grounds, or what philosophical grounds are. Finitism, for example, is said to be a regulative attitude. Yet finitist methods were developed for the purely mathematical purpose of avoiding the paradoxes of set theory. And if finitist methods are pursued instead for their own sake it is even more obvious that the motive is a mathematical one. Nor is it clear that logicism, formalism, and intuitionism are recommendations that certain sorts of mathematics be developed. This account does not apply to logicism. It is not a recommendation but a view that something is the case. It is not a view that more mathematics of a certain kind should be produced but the view that what there is can be derived from 'logic'. Intuitionism recommends that only constructive proofs be used but this recommendation is derived in its turn from a view that something is the case, namely, that non-constructive proofs generate the paradoxes of set theory. Formalism in Hilbert's sense is another proposal for avoiding all possible paradoxes: consistency must be guaranteed by finitist proofs. The search for finitist consistency proofs is recommended because it is thought that this is the only safe way of avoiding contradictions. None of these views is simply a recommendation that a certain sort of mathematics be produced.

Although the discovery of the paradoxes of set theory is described as 'one of the most important and fruitful events in the history of mathematical logic and the philosophy of mathematics' (p. 44) the treatment given to them is hardly in accordance with this estimate. For no systematic account of the paradoxes is given though they are occasionally referred to. There is no mention at all of the effects of the Richard paradox on the work of Russell, Ramsey, Church and Gödel. The liar paradox is mentioned only once in connection with so-called Gödelian self-reference. In Körner's view the paradoxes of set theory arise from an uncritical use of the actual infinite, and while this is nowhere demonstrated, he deals with the actual infinite at great length. Logicians, he says, make uncritical use of the actual infinite and so cannot claim to have a philosophy of infinity at all. Yet Russell gave three chapters of the *Principles* of 1903 to philosophical discussions of Cantorian infinity. Körner is opposed to the Cantorian use of the actual infinite and supports the finitist proposals of formalists and intuitionists. But it is only reasonable to do so if it is first shown that there is something wrong with the actual infinite, and in particular, that it does generate the paradoxes. This is assumed but never shown.

Körner maintains that mathematics cannot be derived from logic. For in his view mathematics asserts the existence of mathematical objects while logic does not, and statements to the effect that there are such things as Euclidean points cannot be derived from statements which do not assert the existence of anything at all. The trouble with this view is that it requires a new and dispensable use of the verb 'to be'. For according to Körner, mathematical objects, unlike zoological objects, spring into being when their existence is consistently asserted. Further, though statements like 'there are Euclidean points'

and 'there are no Euclidean points' are incompatible, in his view neither of them need be false. Now while it might be possible to carry through such a use of the verb 'to be' there is no need to try. We can already say what we want to say about mathematical objects by speaking hypothetically.

Körner complains of each of the logicist, formalist and intuitionist movements in turn that they do not deal with applied mathematics. But why should they? They set themselves to deal with pure mathematics and they are free to do so. His own account is that when pure mathematics is applied inexact concepts are substituted for exact concepts and vice versa. Now while Körner pays a great deal of attention to the distinction between exact and inexact concepts, he pays none at all to the crucial notion of substitution. Nor does he make any use of Curry's analysis of this concept. Inexact concepts allow of neutral candidates, exact concepts do not. Körner distinguishes between inexact concepts and those which are obscure. For the latter have no clearly determined use while the former have. They admit of neutral candidates in a clearly determined way. A neutral candidate is defined as one which might be taken by one person to be a positive candidate, by another person to be a negative candidate (p. 160). It is a candidate about which each person is free to make his own decision. So an inexact concept is one which at some point or points fails to give an unequivocal directive: an exact concept one which never fails to do so. It is clear that on this account any inexact concept can be made exact by obtaining general agreement on what are at present its neutral candidates. But if inexactness can be removed, why should it be laid down as an ineradicable feature of perceptual concepts? Yet the whole of Körner's analysis of applied mathematics turns on the assumption that the gulf between exact and inexact concepts is fixed eternally.

JOHN TUCKER

*Social Principles and the Democratic State* By S. I. BENF and R. S. PETERS. George Allen & Unwin Ltd, London, 1959. Pp. 403. 32s.

ACCORDING to the blurb on the dust-cover (but without support from anything in the text) "the authors of this book are trying to make explicit the social principles which underline the procedures and political practice of the modern democratic state". This is what long stretches of it do seem to be doing, and a reader may regret that it was not in fact made the controlling purpose of the whole. It is something which might be done with a high degree of objectivity—to show (as it were in a Kantian "transcendental deduction") what valuations and what hierarchy of valuations are required to justify the arguments used by all the parties in present-day political discussions in this and similar countries, leaving it in the end to the reader to decide whether he himself accepts that hierarchy of values, and thereby understand better either why he adopts the political position which he does or alternatively why it is that he finds all contemporary politics unsatisfying.

But this is not the plan followed. The authors, one of whom is a philosopher and the other a political scientist, tell us in their preface that their original aim was to supply "a text-book which takes account of recent developments in philosophy without being too remote from the

institutions of the modern welfare state" for the use of the "great number of students who take Social Philosophy as part either of an Honours Degree or of a Diploma in Social Studies Sociology. or Public Administration". The book, as it has emerged, is divided into three parts, of which the first is the most "philosophical", being concerned with what the authors call "forms of social regulation", particularly the nature of Morality and Law. The second part is concerned with the familiar high-sounding "social principles"—Equality, Justice (with appended chapters on Property and Punishment), and Freedom. The third part is "Political Theory" in the specialist's sense—the State. Sovereignty, Political Obligation, and so on—culminating in the expected defence of Liberal Democracy. Finally there is an appendix on "International Relations". Most of what is said is quite good sense, but it is also rather dull (particularly to philosophers, who usually like a little provocation). Some of the blight of the textbook clings to it—the anxiety of the conscientious author lest innocent minds should be led to accept as established fact anything actually controversial—and, since nothing is uncontroversial in politics, here a second-best course has to be followed, which is to adopt a middle-of-the-road position such as will offend as few people as possible. In this book it is roughly the position of the right wing of the Labour Party, so muted as to be possibly acceptable to the left wing of the Conservatives.

Philosophers will naturally be most interested in the philosophy, but this is what is least satisfactory. In the early chapters the fashionable antithesis of "prescriptive" and "descriptive" is tossed around in a way which may sometimes puzzle even the professional and would surely be quite incomprehensible to simple students for whom this "Social Philosophy" was the first philosophy they had ever met. The authors themselves describe their position as "a cautious Utilitarianism which takes full account of the principle of impartiality". In fact the Utilitarianism is very much watered down and the notion of "impartiality" heavily overworked. Utilitarians of the tradition have generally understood that it is important to decide what is the "good" or "happiness" or whatever they have called it that is to be promoted, but here it seems that "impartiality" is all. We are told that to say that the state should "seek the common good" is only to say that it should "attend to the interests of its members in a spirit of impartiality", but, if we are told anywhere what are and what are not anyone's "interests", it is so lightly stressed as to leave no impression. (It would seem that when Keete flogged the Eton boys "for their good", he "sought their common good" so long as he flogged them all "in a spirit of impartiality".) The account of "authority" which Dr Peters presented to the Joint Session at Southampton in 1958 appears again, and anyone who found it confused and confusing then will not find it any less so now, but less is made to hang upon it than might have been expected, because the whole "authoritarian" side of politics is here very much played down. The discussion of punishment, though it makes some sound points by the way, is vitiated by the assumption that, because a book of this kind is properly most concerned with the functions of criminal courts, prisons and other such organs of the state, therefore nothing but this is what the philosophers of the past have had to consider in their accounts of punishment, when in fact they had in view everything from slapping with the parental slipper to consignment to hell-fire.

The authors are in some danger of failing O level English Language in G.C.E. for inability to distinguish the uses of "principle" and "principal". Otherwise the only misprint I have noticed is an intrusive dash in the title of Spinoza's *Tractatus Politicus* in the footnote to page 307

A. M. MacIVER

*Wahrheit und Methode.* By HANS-GEORG GADAMER Tübingen, 1960  
Pp 486, ca £3 Grundzüge einer philosophischen Hermeneutik

PROFESSOR GADAMER (Heidelberg) is a well-known scholar in Greek and Modern Philosophy in his own country. In this book which reads like extensive Gifford Lectures, he gives a summary of his life-long studies by unfolding a new concept of philosophy.

The sub-title indicates the philosophical tradition which gives the framework to the thesis Schleiermacher, a theologian of the Romantic period in Germany who grounded his theology in a pan-theistical system, and Wilhelm Dilthey, following his footsteps two generations later, coined the term "Hermeneutics" or "Geisteswissenschaften". It may at best be compared with the English understanding of the "humanora"; the "Moral Sciences" although "Geisteswissenschaften" are distinguished and developed against the static methods which the latter share with the rationalistic and empiricist traditions of eighteenth-century philosophy. Dilthey's ambition was to develop a method of understanding history and the arts more adequately than was possible by application of the methods useful for the sciences. He understood history as an outcome of the forces of "Life" and based his method, an intuitive revival of historical texts, philosophically by assuming a resemblance of all human affairs resulting from their participation in that ground which he called "Leben". Thus his method went below the classical separation between 'cogitans' and 'cogitatum' and pointed to a prior common basis. History, from which stems this separation, a separation which Descartes' philosophy, modelled on the methods of science, declared to be the only reliable method of inquiry.

This concept of 'Understanding' (Verstehen) Gadamer traces back to the Roman concept of 'sensus communis'. It does not inherit the capacity to subsume a particular case under a general law. Renaissance scholars and Protestant theologians developed this sense to authorize direct inquiries into the meaning either of antique literature ("ad fontem" was the battle-cry of the humanists) or of the words of the Bible. In both cases the human mind freed itself from the bonds of deductive reasoning. Paralleled to Vico, Shaftesbury developed "wit" and "humour" as modes of common sense behaviour which Thomas Reid systematized in his philosophy of Common Sense. Bergson's "bon sens" is based on this "sensus social" as well.

Although Shaftesbury's, together with Edward Young's, influence in the Germany of the eighteenth century was considerable, the English-French born meaning of 'common-sense' did not enter German thinking. Political conditions were not yet developed enough to take in this emblem of liberalism. Deprived of its political frame it enters into the systems of philosophy and re-appears there as "Urteilstkraft", power of judgement. Oetinger, a German pietist, raises common sense into a Baconian "ars inveniendi", drawing largely from Shaftesbury. Kant devaluates it to the juridical subsumption of a given case under a general rule correspondingly.

opposing it as "vulgar sense" (*Gemeiner Menschenverstand*) to the truth-finding Reason. But in the aesthetic theories of the age common sense re-appears as 'judgements of taste' (*Geschmacksurteile*). This aesthetic aspect of common sense gives the first focal point for Gadamer to develop the critical Hermeneutics. Through a phenomenological analysis of "games" he unfolds the aesthetic apprehension of things and events underlying our theoretical knowledge of them. Thus the Arts achieve supremacy over the sciences: as means of expression over reduced and abstract propositional knowledge. Kant's 'subjectivisation' of aesthetic experience must be overcome in favour of Hegel's understanding of the Arts as an aspect in the revelation of spirit in history. Now the understanding of the meaning of arts will give us an unbroken knowledge of universal truth.

The other origin of hermeneutics as a philosophy is found in Gadamer's criticism of the philosophical foundations of the works of the German historians Ranke and Droysen. The examination of their 'historic' shows a pantheistical background against which their understanding of history is justified. Gradually this pantheism underlying the understanding of history is removed in the second part of the last century and gives way to a more materialistic concept of the whole as an organism. This period in the history of the understanding of the arts is represented by Dilthey. Dilthey however has not succeeded in distinguishing clearly between psychology and hermeneutics. Husserl, parallel to Dilthey, as Gadamer thinks, developed this pattern, called "Life" in Dilthey, by exposing the distinction between meaning and thought in his later books. Husserl remains a transcendental subjectivistic philosopher throughout. He does not succeed either in overcoming the antinomies between the acting cogito ("ego" cogitans) and the cultural world which is an infinite reality that never could be unfolded totally out of the isolated subject.

But Husserl's inquiries into the 'intentionality' of the mind reveal the duration of time prior to the atoms of successive events in the consciousness. This discovery will later give the pattern for Heidegger to exhibit the "historicity" of human existence from the radical finitude ("Endlichkeit") of man's life. This "Endlichkeit" gives historicity to all human endeavours to find truth. It means the denial of the idealistic insistence upon a conceptually conceived realm of eternal universals and bases knowledge in the factual existing world.

Gadamer who aims to apply Heidegger's fundamental ontological analysis in the construction of his Hermeneutic, emphasizes mainly the methodological aspect in Heidegger's scripts in order to transfer this into his own foundation of Hermeneutics.

From Hegel and Heidegger Gadamer develops the notion of language as an all-inclusive universal. "Thought is language" This groundwork which language provides for thought Gadamer further exposes, and defends it against the Platonic tradition of degrading the body of the spoken language over and against conceptual thinking by a most subtle interpretation and criticism of Plato's *Cratylus*. This internal criticism, revealing the embedded dialectic in Plato's rejection of common language, would alone be enough to show the penetration of Gadamer's method of criticism.

This ontological place which language now occupies for the finding of truth enables Gadamer to state, in principle, an infinite concept of knowledge. If words not only what methods, concepts or propositions mean, bear the essence of things and events, all that can be said, can also be



known. Thus Gadamer, by grounding the idea of an all penetrative knowledge in language, revives Hegel's claim for universal faith in the body of languages. In the most condensed form language is present in poetry, thus showing the close connection which poetry, prior to the sciences, has to knowledge.

Largely in defence of Plato on the other hand Gadamer advocates the dialogical way of finding truth. The dialogue which people may engage in about something in question would reveal that the process of speaking together proceeds independently of individual intentions to give the discussion a wanted direction. Language, not the 'intentionality' of the individual minds, possesses all thoughts in advance and would guide our thinking to results unknown beforehand. It is here that Gadamer sympathetically follows R. G. Collingwood's "Logic of question and answer" in his *Autobiography*, a criticism of the Oxford Realists and their neglect of history (p. 352). Language thus proves to be the element of communication. But language would be insufficiently understood if given the usual conventionalistic definition. Words are not conventional signs and symbols but rather expressions of the disguised truth of being. To back this ontology Gadamer follows the theological interpretation of the saying "His Word became Flesh", showing that language binds the 'cosmos' together in the human person. So hermeneutics, which is the work of representing the past that our minds inherit through tradition, achieves the rank of a *prima philosophia*. It is the fount of knowledge of truth.

This foundation of knowledge in languages, which Gadamer, unlike for instance Erich Unger in his "Gegen die Dichtung" (1925), does not unify into the ideal of a single archaic language, is certainly tracing the humanistic tradition in which Gadamer, together with some other contemporary German philosophers, participates. He points out that to somebody surrendering to it hermeneutics would mean an exercise in acute speculative interpretations.

There may be many a hint for criticism in this philosophy from points of view external to Gadamer's own. I may be allowed to point out only one which, to me, seems to follow from thinking through his own idea of language as a universal of all universals. Language thus understood appears to me like an endless ocean in which no light-ships nor coasts could be seen. Is this insight into the essence of meaning which language provides for the inquiring thought really the solution of the problem of truth? Is it not true that the activity of mind which may be of necessity to enquire the meaning of a text, creates already an intention which points beyond the enclosure of words which will mean words again? And if this activity were constitutive of hermeneutics would this not place the 'cognito', as a sheer activity, outside language, an activity which it would mainly have to direct upon the context in question? And if this were accepted would the linguistic monism which seems to me implicit in Gadamer's critical hermeneutics not be interrupted at this source, through this activity of the mind which would have to be opposed ontologically to language in some way? I should not like to continue these speculations here as Gadamer's philosophy stands as much in the tradition of Continental, especially German philosophy, of the last fifty or eighty years that a constructive criticism would have to be much more elaborate. But, even those who may disagree entirely with Gadamer's thoughts would find in the book a comprehensive groundwork of philosophy, supported by an admirable knowledge not only of the history of Greek and modern ideas but framed also with a philosophically interpreted knowledge of all the branches of science and art thus truly following

Hegel's *Philosophical Encyclopedia of Knowledge* Although I would not say that Gadamer's book could be read very easily he certainly does not present to his readers as many obstacles as the unusual terminology of his teacher Heidegger

H J SCHÜRING

*The Moral Philosophy of Richard Price* By LENNART AQVIST (Lund · CwK Gleerup, Copenhagen : Ejnar Munksgaard. Library of Theoria, No. 5. Pp 214. No price given )

LOCKE once remarked, in effect, that men could reason correctly before Aristotle invented the syllogism. Similarly, one might have supposed that philosophers could express themselves adequately before Russell and Whitehead wrote *Principia Mathematica*. Mr Aqvist, however, is over-impressed by a technique of exposition which is highly successful in logic and mathematics but, except when used with the greatest discretion, out of place in moral philosophy. In consequence, what might have been a competent but unexciting discussion of some of Richard Price's main tenets—chiefly the epistemological ones—is rendered almost unreadable. This is partly due to a certain irritating use of unnecessary technical terminology, such as 'HP-deontic', '(2')-action', 'P-substitution', and by Mr. Aqvist's inability to talk about actions or properties, but only about 'an action, A' or 'a property, P'. The chief reason for the great difficulty any reader must have in following Mr. Aqvist's arguments, however, is this. He has, presumably partly to avoid the necessity of repeating himself, and partly to state things concisely and in a way which makes them stand out, expressed all the contentions or definitions he wishes to discuss in separate paragraphs, and numbered them "B" or "1" or "(1)", when variants on them are subsequently produced, which happens frequently, these are then numbered "B'", or "1'" or "(1)'" . These must occur at the rate of about half a dozen to the page. The continual occurrence of new paragraphs prevents the reader from getting into a stride, and the fact that every contention discussed, however trivial, is dignified by a separate paragraph, means that the important ones get lost. Worse than either of these faults, however, is that once something has been labelled, it is subsequently referred to by its letter or number, and since many sentences refer without page references, to some three of these contentions, which sometimes occur a long way back, the reader has, by the time he has located and understood them all—which sometimes means referring back to still other numbered contentions—forgotten what it was to be proved, and, unless he is patient, ceased to care. The trouble is, presumably, that Mr Aqvist is so interested in and so familiar with the ins and outs of what he is saying that he has completely lost sight of the difficulties which might be experienced by a reader. This is a pity, for his observations, though often niggling and barbarously expressed, are frequently acute and sound. Most of what he has to say, however, is simply not important or original enough to warrant the labour involved in mastering it, except to a few specialists. Anyone unacquainted with Richard Price's work would get a very queer idea of it from reading Mr Aqvist's book, and would, indeed, be in for a very pleasant surprise, for Price's solemn but serviceable prose style is, compared with that of his expositor, a masterpiece of lucidity, vivacity and wit.

JONATHAN HARRISON

*La Politique Morale de John Locke* By RAYMOND POLIN Paris: Presses Universitaires de France, 1960 Pp 320 14 NF

THIS is an investigation of the nature and degree of synthesis between Locke's moral and political theory. Its significance lies in the author's challenge to four leading theses advanced by Peter Laslett in his recent edition of Locke's *Two Treatises of Government*. Admittedly, Professor Polin nowhere explicitly argues against Laslett and he may not be aware himself of the full significance of his project. It is clear, nevertheless, that he has read Laslett's Introduction in manuscript and also knew his previous articles adumbrating the novel interpretations.

The points on which Polin challenges Laslett's views are these: (1) He insists that Locke wrote *Two Treatises* not merely in response to a particular political situation but as a philosopher, i.e. in the form of an analytic argument, a statement of universal principle. (2) He refuses to believe that what Locke said in his *Essay concerning Human Understanding* had no bearing on his political theory, he attempts to show, for instance, that there is an essential connection between his philosophical doctrine of power on the one hand and his notions of will, reason, happiness, and liberty on the other. (3) As he looks upon Locke's early Oxford writings on the civil magistrate and on natural law as important stages of his thought, he does not like Laslett, regard his mature philosophy as exclusively the result of the intervening influence of Shaftesbury. In this connection, he seeks to put an end to the controversy concerning the degree of admixture of authoritarian or Hobbiist elements in Locke's views: he shows that, in principle, there was no  *volte-face* , no conversion between his early writings and his subsequent notions of liberty and toleration. (4) As the title of the book indicates, he endeavours to treat Locke's views on ethics and politics, though obviously falling into two distinct domains, as inseparable from one another, and he looks upon his doctrine of natural law as their common basis. Such an approach must pose the question, never previously answered or even raised, whether Locke would have regarded political theory like ethics as a demonstrable science. While Laslett professes to see an affinity between Locke's discussion of politics and empirical medicine, one might justifiably argue that Locke thought political theory, which he distinguished from political action or prudence since these are dependent on experience and analogical reasoning, to be founded upon fixed definitions (cf e.g. *First Treatise*, paras 23, 108, 109, *Second Treatise*, para 52) and hence, by means of deductive reasoning, capable of demonstration.

In the main, I think, Polin's exposition is on the right lines, except perhaps for his too frequent attempts at a rapprochement between Locke and Kant. He is particularly good in his accounts of controversial questions, such as Locke's theory of the state of nature, which he succeeds in making unambiguous without either too much divorcing it from, or assimilating it to Hobbes's conception. The way in which he co-ordinates the different features of Locke's notoriously complex moral theory, for instance his hedonism and rationalism, is likewise excellent (pp 48-61). A further advantage is that of all recent authors on Locke's moral and political philosophy in this country, France, Italy and the United States, Polin has paid the closest attention to the relevant new manuscript material in Oxford. The flow of publications since this material first became available is indeed gratifying. There are signs that more are on the way.

*La Logique de la Simplicité.* By ANDRÉ LAMOUCHE : Dunod (Paris, 1959).  
Pp. xii + 536 + xxi.

THIS book illustrates very clearly the gulf that now separates Anglo-Saxon philosophy from much that is done on the Continent. It is supposed to be a treatise on the logic and philosophy of science, introducing a new kind of logical principle. In all its 536 pages I have not been able to spot an example of any kind, either in logic or in science, to illustrate the operation of this principle. This makes a good deal of the text impenetrable.

The first part, 'The Evolution of Logic', is a series of *obiter dicta* about logic taken from almost every logician since Aristotle. No illustrations are given of any of the logical systems mentioned. In the second part the author propounds his logic of simplicity. In effect he is saying that both the physical universe and the human mind are governed by principles of simplicity. It is not made clear whether this simplicity is to be taken as paucity of elements or simplicity of structure, a distinction vital to any discussion of this sort. By pursuing simplicity in our thinking we are supposed to be guaranteed a measure of understanding of the physical universe. Again no examples of a successful instance of such a pursuit are given. The theory is claimed to be proved by being the only possible explanation of 'the "marvellous correspondence" between Mathematics and Physics'. What this marvellous correspondence is the author does not say.

As a characteristic instance of the method of argument adopted in the book I quote a key passage in the exposition of the theory, taken from page 489. 'The first [the Logic of Simplicity] reveals the norms according to which the two logical functions of assimilation and composition act and interact in the specific dynamism of the human intelligence. The second [the Methodology of Concord] of secondary strength, analyses the cycle of more complex operations, through which the certainty of science emerges from a combined action of psychic functions (or psycho-physiological functions) which insures the double "presence in the world" of human consciousness: its presence in the exterior world and its presence in the interior world'. Faced by a complete absence of examples we can hardly say that M. Lamouche is wrong, but equally he cannot expect us to think him right.

R. HARRÉ

*An Introduction to Metaphysics.* By MARTIN HEIDEGGER, translated by RALPH MANHEIM. Yale University Press (London: Oxford University Press), 1959. Pp. xi + 214. 22s. 6d.

I SUSPECT that this book does not show Heidegger at his best. In some of his other writings, behind a bristling barricade of abstract-looking terminology, Heidegger is sometimes, just discernibly, discussing some concrete topic—people, for instance, under the name of "the *Dasein*"; and on such topics he sometimes has things to say which are not true exactly, not always precisely meaningful, and never clear, but also not uninteresting. Admittedly, on the general topic of the human predicament novelists and dramatists usually do better; but some philosophers do not do badly, and Heidegger is at times of their company.

But the theme of the present work is "the question of Being"—a topic (if it is a topic) of maximum abstractness, and one on which it is really not possible to be dimly dramatic, enigmatically exciting, rhetorically effective, or darkly and paradoxically powerful. Here bombast, for the want of appropriate subject matter, achieves nothing at all. The reader is told, over and over again, that the two fundamental questions are "Why is there anything rather than nothing?" and "How does it stand with being?", all that is offered by way of answer is that being is not *becoming*, is not *appearing*, is not *thinking*, is not "the Ought", and the author's strenuous insistence that asking questions is a far more vital activity than answering them is poor consolation. One feels entitled to expect more nutrition from so thick a gruel, particularly in view of the gigantic pretentiousness with which it is served up.

It should be added that the present volume is a translation from the German edition, dated 1953, of lectures delivered by Heidegger in 1935. One round compliment to Nazism has bravely (or perhaps inadvertently) been allowed to stand (p. 199).

G. J. WARNOCK

*The Ethical Animal* By C. H. WADDINGTON. London. Allen and Unwin, 1960. Pp. 218. 25s.

PROFESSOR WADDINGTON'S book touches on a great variety of topics, some of them rather disconnected. He does, however, advance a central thesis, which is roughly as follows. Evolution has a definite and discernible direction, which is not accidental but inherent in the nature of the evolutionary process. Human evolution is principally cultural rather than physical, and depends on the transmission of information from one generation to the next. The human readiness to accept transmitted information "is founded on the formation of 'authority-bearing' systems within the mind which also result in the human individual becoming a creature which goes in for having beliefs of the particular tone we call ethical." Thus ethical beliefs have the function of "making possible human evolution according to the mode which it is following." Ethical beliefs can therefore "be meaningfully judged according to their efficacy in furthering the general evolutionary direction".

This is an ingenious and interesting thesis. The main difficulty about it is its vagueness. The comparison between cultural evolution and genetic evolution is rather vague. The concept of "function", which plays an important part in the argument, is explained thus: "When we assign a function to something we assert . . . that it forms part of a causal network, and the results of the causal network when observed over the range in which they are expressed exhibit some general property", and this is extremely vague. It is asserted that cultural learning is dependent on the adoption of ethical beliefs, but the nature of the connection is not explained, and, while Waddington produces evidence to show that his "authority-bearing systems" do get formed in the mind, he produces none to show that their formation is necessary to the acceptance of transmitted information.

As to his suggested criterion for judging ethical beliefs, there are two obvious difficulties. Firstly, in order to apply the criterion, we need to know what the "general evolutionary direction" is, and here again there is vagueness. In Chapter 11 Waddington speaks of a trend towards

greater efficiency, for which there is ample and familiar evidence. In Chapter 16 he speaks of a trend towards richness of experience (a very different matter), for which I think satisfactory evidence would be hard to produce. But when, at the end of the book, he comes to the discussion of some current moral issues, though he has many sensible things to say, he does not manage to get any definite guidance from his ideas of evolutionary direction.

Secondly, granted that there is an evolutionary direction, why should we approve or further it? Waddington's reasons for doing so are not clearly stated, but I think he means, not that we should approve and further the course of evolution as such, wherever it may lead, but only in so far as it leads to consequences in themselves desirable. For, while he admits that there are evolutionary sequences which lead to no improvement ("stasigenesis" and "cladogenesis"), he takes account in his moral theory only of those which do lead to improvement ("anagenesis"). If this is his position, it presupposes a standard of value independent of the actual course of evolution.

C. H. WHITELEY

*Metascientific Queries* By MARIO BUNGE Springfield, Illinois · Charles C Thomas, 1959. Pp. xiv + 313. 51s

THIS is a collection of essays on the conceptual problems of physical science. All the essays have been published before, mostly in South and North American journals; they are reprinted here in a revised and enlarged version. The first few chapters deal with the nature and function of methodology ending in a general discussion of metascientific theory. Then follows a chapter on computers. The remaining four essays are mainly concerned with the philosophical interpretation of quantum mechanics.

The range of problems discussed by the author—from the nature of science to the time reversal in elementary particle theory—is so wide that it is impossible to give a detailed criticism here. The author argues from the viewpoint of what he calls 'scientific materialism'. His arguments are sometimes rather polemical and directed mainly against the common philosophy of modern physicists, that is, logical positivism and empiricism. Some of his attacks are quite justified; but the author does not realise that his scientific materialism is as much an arbitrary epistemology as is positivism. Modern science has long outgrown the -isms of classical philosophy. This is the reason for the philosophical behaviour of some physicists—which the author finds so strange—when they jump from idealist to realist arguments in their explanations. Here lies the origin of the endless disputes concerning quantum mechanical reality. The various -isms of epistemology correspond to the methodology of inductivism which has been recognised to be insufficient for modern science ever since Einstein first pointed it out in his Spencer lecture of 1933.

Still, the author's discussion of standard philosophical problems, e.g. of knowledge, of determinism, of individuals, etc., is always interesting. He shows how the problems arise from scientific theory and he brings to them a lively and fresh treatment. The book is quite popular and may be read with profit by philosophers who want to learn something about the metaphysics of today.

ERNEST H. HUTTEN

*Oxford Essays in Jurisprudence* Edited by A. G. GUEST. Clarendon Press Oxford University Press, 1961. Pp xviii + 292 25s

THIS book consists of ten essays by members of the Oxford Faculty of Law.

In the first two essays P. J. Fitzgerald and H. L. A. Hart move beyond the orthodox theory of action in Anglo-American jurisprudence, namely, that voluntary action consists of the desire for a muscular contraction or perhaps the desire plus the contraction. Fitzgerald considers the circumstances in which courts actually accept that someone's acts or omissions were 'involuntary'. His conclusion is that justice and deterrence both demand that an agent should be held responsible only where he could have controlled his movements, except where the lack of control could have been avoided by an earlier action which *was* in his power, or (in Civil Law) where he could have avoided creating a situation of exceptional danger to others.

The orthodox theory of action will lead one to analyse any act for which the agent can decently be held responsible into (i) the muscular contraction, (ii) the desire for the muscular contraction, and (iii) foresight of the consequences of the muscular contraction. But 'criminal negligence' includes the absence of (iii). Is it then mere barbarism to punish, *e.g.* careless driving? Hart answers that if we are going to punish at all, there is no reason in principle why we should not punish negligence, since we can have just as good evidence for asserting that someone could have avoided being negligent as for asserting that he could have avoided deliberately doing something. In both cases it is a matter of estimating whether he had the relevant capacities—a modicum of intelligence, self-control, etc.

A. W. B. Simpson asks two questions. Firstly, can a court (*e.g.* the House of Lords) sensibly *decide* that in future it will be bound by its precedents? He answers that this is possible in the sense that the court is then bound by its precedents until it decides otherwise, just as a person may decide to act on a certain principle until further notice. Secondly, what does it mean to say that a court is so bound? He answers that the court must either follow or distinguish any previous cases which might be regarded as relevant.

A. G. Guest discusses 'Logic in the Law', rejecting the popular antithesis between 'logic' and 'life' or 'experience'. But he fights straw men. Of course, the judge can always make his decisions 'logical' by suitably defining his terms, but the question is how should he decide which way to define them? Crudely and shortly, where he cannot simply subsume the present case under the obviously appropriate legal category, he must draw analogies by (a) considering the detailed similarities and dissimilarities between the present case and the unproblematical paradigm cases, or (b) saying that the object served in treating the paradigm in the legally prescribed way will (or will not) be served by treating the present case in the same way. (a) and (b) may lead to different results and it is here that the conflict between logic (= a) and life (= b) arises.

R. F. V. Heuston ably sets out and defends the 'new theory' of sovereignty, according to which 'the sovereign' is the name for a body of men going through a certain procedure—not simply a body of men. If the procedure is changed, so is the sovereign. In an extraordinary final paragraph, Heuston appears to claim that the 'new theory' of sovereignty provides a painless Common Law substitute for a Bill of Rights. In truth, though, it still does nothing to stop Parliament from killing all blue-eyed babies. It only says that if the 'Queen-in-Parliament' were

re-defined for this purpose as, say, two-thirds of the Commons, then the measure would have to be passed by that majority.

N. S. Marsh contributes a long and absorbing account of recent work on the concept of the 'Rule of Law' which has been done under the auspices of UNESCO and the International Commission of Jurists. The agreed conclusions of these massive international efforts are hardly striking or original. More important is the fact that such a large measure of agreement among jurists from different countries was achieved, both on what can be generalised about and what cannot. For example, there appears to have been a general recognition that the same job of protecting rights may be done by quite different kinds of institutions.

G. Marshall asks whether some matters are by their nature suitable or unsuitable for judicial (particularly as against administrative) decision. He reviews a number of criteria which have been proposed, finds them wanting and claims that the allocation of decisions is a job for the legislature, since the real question is whether or not the decision is to be subject to ministerial control. But to reformulate the terms of a problem is not necessarily to eliminate it, and we must surely ask, "Aren't some matters inherently more suited to ministerial control than others?"

The three remaining essays are of a more technical nature. J. F. Lever does some tidying up in the law of Torts, D. R. Harris follows out the labyrinthine complications of 'The Concept of Possession in English Law'; and A. M. Honoré analyses 'Ownership'. In place of 'ownership' as the name for a 'bundle of rights' varying capriciously from one legal system to another, he offers us a paradigm of 'full' or 'liberal' ownership. Particular legal systems may recognise only something short of 'full' ownership, but most, he suggests, allow 'full' ownership over at least some goods.

There are many misprints, and points 4 and 5 on page 282 are referred to on page 283 as points 5 and 6 respectively.

B. M. BARRY

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## X.—NOTES

### ARISTOTELIAN SOCIETY

The next General Meeting of the Mind Association and Joint Session with the Aristotelian Society will be held at Leicester from 13th to 15th July 1962. Notices and application forms will be sent automatically only to members of Mind Association resident in Europe (section (a) of the list of members). If any member in section (b) of the list wishes to attend the meeting he is asked to write to the Treasurer, Mr J. D. Mabbott, at St. John's College, Oxford, before the end of April and the papers will be sent to him.

Any member who is unable to attend but would like to obtain the volume of papers to be read at the meeting should write in July to the publishers (Messrs. Harrison and Sons Ltd., St Martin's Lane, London W.C.2).

The programme for the Joint Session is as follows

- |                |      |   |
|----------------|------|---|
| Friday, 13th   | p m. | Inaugural Address: 'Contextual Implication and Ethical Theory', Professor P. H. Nowell-Smith. <i>Chairman</i> , Professor A. M. MacIver                           |
| Saturday, 14th | a m. | (1) Symposium 'Mathematical Discovery', Professor R. L. Goodstein and Dr I. Lakatos. <i>Chairman</i> , Professor W. C. Kneale                                     |
|                |      | (2) Symposium. 'Criteria for a Psycho-analytic Interpretation', Mr B. A. Farrell and Dr J. O. Wisdom and Dr P. M. Turquet. <i>Chairman</i> , Professor C. A. Mace |
|                | p m. | Symposium. 'Communication and Expression in the Arts', Professor E. H. J. Gombrich and Dr Ruth Saw. <i>Chairman</i> , Mr Bernard Mayo.                            |
| Sunday, 15th   | a m. | (1) Symposium 'Prudence', Mr J. D. Mabbott and Mr H. J. N. Horsburgh. <i>Chairman</i> , Dr R. S. Peters   |
|                |      | (2) Symposium: 'Subjunctive Conditionals', Mr P. Alexander and Dr. Mary Hesse. <i>Chairman</i> , Mr R. Harré.   |
|                | p m. | Symposium 'Claims to Knowledge', Mr G. J. Warnock and Mr L. J. Cohen. <i>Chairman</i> , Dr A. C. Ewing  |

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**PHILOSOPHY IN THE MID-CENTURY** edited by R. Klibansky  
The fourth volume of this set was published by La Nuova Italia Editrice in 1960, after the receipt by the Editor of MIND of the review of the first three volumes. This review was published in April 1961.

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The First Colloquium of the *Société britannique de philosophie de langue française* will be held from Friday, March 30, to Sunday, April 1, 1962, at Canterbury Hall, Cartwright Gardens, London.

Papers include -

"The Ontology of Gabriel Marcel" by Professor I. W. Alexander ;

"Questions cartésiennes" by Professor F. Alquié ;

"Quelques aspects de la philosophie de G. Bachelard" by P. Gnani ;

"The Phenomenology of Merleau-Ponty" by C. Smith.

Particulars may be obtained from the Hon. Secretary, Axel Stern, The University, Hull

### MIND ASSOCIATION

Those who wish to join the Association should communicate with the Hon. Treasurer, Mr J. D. MABBOTT, St John's College, Oxford, to whom the yearly subscription of Twenty-five Shillings (payable in advance) should be sent. Cheques should be made payable to the Mind Association. Members may pay a Life Composition of £27 instead of the annual subscription. The annual subscription may be paid by Banker's Order, forms for this purpose can be obtained from the Hon. Treasurer.

The Hon. Secretary of the Association is Professor A. R. WHITE, The University, Hull.

Members resident in U.S.A. are requested to pay the subscription (\$4.00) to the Hon. Assistant-Treasurer, Professor Alan Anderson, 993 Yale Station New Haven, Conn.

Those who join the Association after previously buying *MIND* direct from the publishers or through a bookseller are asked to inform the Hon. Treasurer of this (and of the name of the bookseller) when they join.

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Contributors of Articles and Discussion Notes should in future always enclose a stamped and addressed envelope. Those from countries outside the United Kingdom should send international stamp coupons to cover return postage.

# M I N D

## A QUARTERLY REVIEW

### OF

## PSYCHOLOGY AND PHILOSOPHY

### I.—EACH AND EVERY, ANY AND ALL

BY ZENO VENDLER

THE theory of quantification is supposed to provide us with a clear model of the logical import of such particles of language as "all", "every", "each", "any" and "some". Even if it is not claimed that the theory can account for all the aspects that are involved in the correct use of these words, it is commonly maintained that the logically important features are well brought out, and taken care of in a manner that surpasses the original in clarity. "Quantification cuts across the vernacular use of 'all', 'every', 'any', and also 'some', 'a certain', etc., . . . in such a fashion as to clear away the baffling tangle of ambiguities and obscurities. . . . The device of quantification subjects this level of discourse, for the first time, to a clear and general algorithm."<sup>1</sup>

As the same text shows in detail, some ambiguities and obscurities are indeed cleared away by the technical devices at our disposal. Elated by this success one is naturally inclined to force all sentences in which these particles occur into the strait-jacket prescribed by the theory of quantification, suppressing thereby, I fear, other aspects, among them logically important ones, that enter into the common understanding of these words. The fact that the theory succeeded in clarifying some logically important points does not show that all the remaining points are of a mere stylistic but not logical interest; the fact that the cake once has been cut with success does not mean that this is the only profitable way of cutting the cake. More in particular, I have reasons to think that the method of lumping "each", "every", "all" and "any" together and treating them as stylistic variants of the same logical structure tends to obscure issues concerning the type of reference, existential import and law-like form of general

<sup>1</sup> W. V. Quine, *Mathematical Logic*, revised edition, Cambridge, Mass., Harvard University Press, 1951, pp. 70-71.

propositions. In the following, therefore, I shall attempt to discern and to exhibit the differences as well as the similarities in the rôle of these particles, which task, surprisingly enough, has never been undertaken yet in a systematic way, at least not to my knowledge.

As we consider the various sentences in which these particles occur, the first difference that strikes us is a grammatical one. "Every" and "each" are always followed by the singular form of the noun, while "any" sometimes, and "all" nearly always, calls for the plural.

Leaving aside, for the time being, the less consistent "any", we shall focus our attention on the difference in this respect between "all" on the one side, and "every" and "each" on the other, and we shall attempt to find the reason behind it. For, as we are going to see, it is by no means a mere caprice of grammar: it is indicative of a difference in the very meaning of these words.

Consider these propositions:

- (a) All those blocks are yellow.
- (b) All those blocks are similar.
- (c) All those blocks fit together.
- (d) The number of all those blocks is 17.

It is clear that (a) is true if and only if the proposition:

(aa) Each (every one) of those blocks is yellow.

is true. Thus here, at least in so far as truth-values are concerned, no difference appears between the functions of these particles

This, however, is obviously not the case in regard to (b), (c) and (d). For, to begin with,

Each (every one) of those blocks is similar.

is an incomplete sentence; the question "similar to what?" remains open. One may try to be more specific:

(bb) Each of those blocks is similar to every other.

Even this version will not do though. If we interpret the relation of similarity as having at least one common characteristic, then it is quite possible that each block be similar to every other without all of them being similar. Goodman's example for an "imperfect community" is sufficient to illustrate the point.<sup>1</sup> Take three elements with characteristics distributed as follows. *ab, bc, ac*. Then, with the given interpretation, any two elements will be similar to each other without all of them being similar, since there is no common characteristic running through the total population

<sup>1</sup> N. Goodman, *The Structure of Appearance*, Cambridge, Mass., Harvard University Press, 1951, p. 125.

Thus while (b) obviously implies (bb), the latter fails to imply the former.

As to (c), the difference is still more marked. There, again, the sentence : Each (every one) of those blocks fits together. does not make sense, and the improved version :

(cc) Each of those blocks fits every other.

once more fails to amount to (c). It is quite possible that each block fits every other without all of them fitting together. Think of L-shaped blocks, any two fitting together to form a cube. Thus (cc) does not imply (c). But, in this case, neither does (c), *per se*, imply (cc) : all the pieces of a jigsaw puzzle fit together without each piece fitting every other. Each, however, must fit some others.

Proposition (d) brings out the difference in the most extreme form. The counterpart :

(dd) The number of each (every one) of those blocks is 17. will not make sense unless an entirely different interpretation of "number of" is invoked, say, being marked with the numeral "17". In this case, of course, there is no logical relation between (d) and (dd) whatever.

What do these examples show ? We have found that while in the case of a non-relational property the difference between the function of "all" and that of "every" and "each" did not register in the truth-values of the propositions in which they occurred, in the cases of certain relational properties that difference, as it were, could be exhibited in terms of truth-values. Of course, exactly that was the point in using these relational predicates. The relations of similarity (with the given interpretation) and of fitting together can apply to the whole set in a *collective* sense, and to subsets (couples) of the whole group in a *distributive* sense ; and the expressions "are similar" or "fit together" do not indicate, by themselves, in which of these senses they be predicated. It is, therefore, up to the quantifier particles alone to decide the issue. Since, however, the collective sense may fail to imply the distributive sense and *vice versa*, that is to say, one respective proposition may be true and the other false, such a difference in truth-values clearly indicates the difference in the meaning of these particles. Similarly, in the last case, the phrase "number of" requires an entirely different interpretation according to whether collective or distributive reference is indicated by the quantifying particle. We can safely conclude then that, at least with respect to a given group of individuals, the reference appropriate to "all" is collective, and the reference appropriate to "each" or "every" is distributive.



Proceeding from the other end we arrive at the same conclusion. Once more, for the given reason, relational predicates provide the best examples;

Every member of the tribe has two wives.

Each item in the store costs \$5.

do not mean that all the members of the tribe (taken together) have only two wives, or that all the items in the store (taken together) are worth only \$5. In all these cases, again, "all" implies collectivity, "every" or "each" distributivity.

Now we understand the reason why "all" calls for the plural, but "every" and "each" go with the singular. Besides, we understand some other peculiarities as well. We mentioned above that while

All those blocks are similar.

is a complete sentence,

Each (every one) of those blocks is similar.

needs a complement. The reason is that similarity, being a relation, requires at least two terms; now "all", with its collective reference, furnishes more than one already; "each" or "every", being distributive, give us only one subject, though, as it were, many times over. No wonder, then, that we are looking for the other term(-s): similar to what?

Again, "all" has an exclusive and characteristic use in connection with nouns that are, in some sense or other, collective by themselves:

All the information we obtained . . .

All petroleum is organic in origin.

All the nation remembered him.

We feel that the use of "all" is almost redundant here: it merely adds an emphasis to the universality of the subject. Incidentally, such contexts are exceptions to the rule; here "all" goes with the singular.

Finally, the very possibility of phrases like "all together", "all over" on the one hand, and "each separately", "every single one" on the other, fits into the picture we succeeded in drawing by more elaborate means.

In the examples hitherto quoted we treated "every" and "each" pretty much alike. Yet, I think, a closer scrutiny reveals some differences here too. These, however, are much too fine to be located by merely comparing truth-values. In order to spot them we have to summon our best feeling for English idioms, and without disdaining help from other quarters, be they

pragmatic or historical. In doing so, at appropriate junctions, I mean to cast a belated look on "all" as well; this might give us a chance of bringing some colour into the logical sketch of the previous section.

Here, once more, we start off with a difference that is, in a sense, grammatical in nature. While the expression "each of them" is correct, "every of them" sounds ungrammatical; one has to say "every one of them". On the contrary, "each one of them" is somewhat redundant. It looks as if "each" already implied "one" and drew our attention to the individual elements in a peculiar way. Indeed, while the sentences :

He came every day.

He came each day.

are both correct (yet we feel some difference),

He came each second day.

He came each three days.

sound odd, the usual forms being

He came every second day.

He came every three days.

The reason seems to be that no day is a second, or a third, day without a reference to other days. Now, then, while "every" considers the days as they are among other days, "each" takes them one by one, as it were without their environment. We may take a hint from the dictionaries; they tell us that "every" comes from "ever each", thus originally it served to *sum up* the distribution characteristic of "each". In this sense, "every" is between "each" and "all". This explains why "every" becomes pompous if the reference class contains only two elements; we have to say

Each of the two . . .

instead of

Every one of the two . . .

Then it is not surprising to find an exclusive rôle for "each" in contexts like

Each in turn . . .

They cost a penny each.

They love each other.

The sides of these triangles are equal each to each.

In the last two examples the rôle of "each" is more akin to that of "one" than to that of "every": what we want to express is a one-to-one relation ("one another") or correspondence ("one to one").

Lest I should be accused of indulging in hair-splitting I now shift my argument to more pragmatic grounds. Suppose I show you a basketful of apples and I tell you

Take all of them.

If you started to pick them one by one, I should be surprised. My offer was sweeping: you should take the apples, if possible, "en bloc". Had I said

Take every one of them.

I should not care how you take them, provided you do not leave any behind. If I say

Take each of them.

one feels that the sentence is unfinished. Something like

Take each of them and examine them in turn.

is expected. Thus I expect you to take them one after the other not *missing* any.

The anticipated response to the first order squares nicely with the collective rôle of "all" we brought out in the previous section. The other two orders are both distributive, yet with a marked difference in emphasis: "every" stresses completeness or, rather, exhaustiveness (remember "ever each"), "each", on the other hand, directs one's attention to the individuals as they appear, in some succession or other, one by one. Such an individual attention is not required in vain: you have to *do* something with each of them, one after the other.

It makes sense to say that all the deputies rose as the king entered the House (like *one man* they rose), it also makes sense to say that every one of them rose at that moment (*no one* remained seated), but it is rather queer to say that each of them rose at that moment. On the contrary, it is more proper to say that each deputy rose as his name was called, than to say that every deputy rose as his name was called.

To summarize: our considerations in this section not only confirm the basic difference between the collective "all" and the distributive "every" and "each", but they suggest a divergence in the respective functions of the last two particles as well. Moreover, as we have seen, some of these differences are not merely matters of grammar and style: they may affect truth-values as well. This result may encourage us to face our toughest but most important task: the examination of the logical behaviour of "any".

The meaning of "any" is a many-splendoured thing. No example, in itself, could suffice to exhibit its wide variety of

aspects. The best we can do is to discover these aspects one by one, isolate them, and then proceed to explain the import of "any" in some of its characteristic occurrences in terms of those aspects previously described.

As a first step, I take up the apple-basket once more. Now I tell you

Take any one of them.

This offer is far less generous than the previous ones : now I do not ask you to take all of them, every one of them, or each of them; I only give you one, though, for sure, the one you fancy. Thus there is some generosity left in this offer too : generosity in the sense of generality. Had I merely said

Take one.

you might test my good will by asking

Do you mean *any* one ?

Now, notice, it is not sufficient to say that the main feature of "any", in such contexts, is the lack of determination. "Take one" lacks determination as well, but, and this is the crucial point, here the determination may still be up to me ; you may sensibly ask back "Which one ?" With "Take any one", it is up to you to do the determining ; here it does not make sense to ask back "Which one ?" Thus while in the former case I merely fail to determine, in the latter case I call upon you to determine, in other words, I grant you the unrestricted liberty of individual choice. It is interesting to notice that the "tone" of freedom connoted by "any" excludes coercion : "Take any" is hardly an order ; it is an offer. "I ordered (forced, compelled) him to take any" or "You must take any" do not make sense.

The point comes out still better in case of a claim or assertion. The assertion :

I can beat one of you.

or

I can beat some of you.

does not amount, by a long shot, to the assertion :

I can beat any one of you.

The first two assertions claim that there is one person (or some persons) among you whom I can beat, but I do not care to indicate who he is (or who they are). The third, however, claims that no matter whom you select from among you, I can beat him.

For future reference, let us call this very peculiar aspect of the use of "any", which, as we saw, succeeds in blending indetermination with generality, *freedom of choice*. This is an essential

feature ; so much so that in situations that exclude such freedom, the use of " any " becomes nonsensical. Suppose you accept my previous offer and take an apple. What can I say now ? Well, for sure, I can say things like

He took one.

He took the one he liked.

He took that one.

but I certainly cannot say

He took any one.

even if you acted on my words : " Take any one ". Thus, again, the main feature of " any " is not merely indeterminateness, for " He took one " is indeterminate enough. " Any " calls for a choice, but after it has been made " any " loses its point.

My original offer .

Take any one of them.

clearly restricted you as to the number of apples you were permitted to take. Nothing prevents me, however, from being more generous : I may tell you

Take any two (three, etc.) of them.

Thus, it seems, " any ", by itself, is indifferent to the size of its immediate scope. This can be shown by another consideration too. If I ask you

Did you take two ?

and, say, you took three, you have to answer

No, I took three.

If, on the other hand, my question is

Did you take any ?

you have to give an affirmative answer regardless of the number you took , you will say

Yes, I took three.

This discloses, then, a new aspect of " any ", which we will call *indifference of size*. " Take any " leaves you free both as to which and how many to choose.

This indifference has a very curious limitation : if I formulate my offer in terms of " any ", there will be an upper limit to my generosity. In case the basket contains, e g, only five apples, I can go as far as asking you to take any four of them, but I cannot, logically, go all the way and ask you to take any five of them. For to do so would render your freedom of choice vacuous and, consequently, my use of " any " senseless. Hence we may conclude that the immediate scope of " any " cannot exhaust the

total population; in other words, "any" never amounts to "every". Let us henceforth refer to this last property of our beloved particle as to its *incompleteness*.

Now we are ready to examine some of the more interesting and more important uses of "any". We hear, more often than we wish to, commercials of the type:

Any doctor will tell you that Stopsneeze helps.

Suppose we are interested in commercial ethics and we want to check up on the sponsor's claim. How should we go about it?

"Well,—you say—obviously by finding out if it is indeed so."

Unfortunately this answer, straightforward as it is, seems to call for something impossible. In order to realize this, I propose to consider two other examples first. Suppose you tell me

Dr. Jones will come tomorrow.

On the next day, after Dr. Jones has duly arrived, I can say

You told me that Dr. Jones would come today and he came indeed.

In other words, it happened as you predicted. Now you say this:

Dr. Jones will tell you that Stopsneeze helps.

This, too, may be taken as a simple forecast: he will tell me this whether I ask him or not. But the obvious sense is somewhat different: it amounts to

If you ask Dr. Jones, he will tell you . . .

The prediction, in this case, is a conditional one. Now I ask Dr. Jones and he answers in the affirmative. Thus I conclude

You predicted that if I ask Dr. Jones, he would tell me that Stopsneeze helps. I asked him and he indeed told me so.

In other words, again, it has turned out as you predicted.

Finally we return to the sponsor's claim:

Any doctor will tell you that Stopsneeze helps.

At the first sight, this seems to be a conditional forecast too:

If you ask any doctor, he will tell you . . .

Now the question is how can I express a favourable result of my checking up on this claim? Well, I might end up with

He said that any doctor would tell me that Stopsneeze helps. I asked Dr. Jones and he indeed told me so.

or

. . . I asked a good many doctors and every one of them told me that Stopsneeze helps.

but, and this is the crucial point, no matter what I did or what I learned, I shall not be able to say

He said that any doctor would tell me that Stopsneeze helps.  
I asked any doctor(-s), and he (they) told me that Stopsneeze helps.

Accordingly, in an important sense, I cannot conclude that what the sponsor said is indeed the case the way I could conclude in the previous examples.

What is the reason for this difference? If we recall what we said above about the freedom of choice of "any" then we realize that, though you can *ask* me to consult any doctor, or you can *claim* that any doctor will tell me such and such, I cannot *report* that I asked 'any' doctor and I cannot *state* that 'any' doctor told me such and such. The contexts "I asked  $x$ " and " $x$  told me" are used to report a *fait accompli*, and such a use precludes the liberty of choice essential to "any"; facts are not free. You can *state* that  $A$  is  $B$ , or that all the  $A$ 's are  $B$ , and, after some investigation, I may conclude: "I have inspected  $A$  and found it to be  $B$ " or "I have inspected all the  $A$ 's and found them to be  $B$ "; you can *predict* that  $A$  will be  $B$ , and, in due time, I may report: " $A$  indeed has turned out to be  $B$ "; but, though you can *claim* that any  $A$  is  $B$ , I can never conclude: "I have seen any  $A$  and any  $A$  has turned out to be  $B$ ."

Ask Dr. Jones and he will tell you . . .

Here "he" refers to Dr. Jones.

Ask any doctor and he will tell you . . .

Ask any doctors and they will tell you . . .

Here "he" and "they" do not refer to 'any' doctor, or doctors, I *may* ask; they refer to the one, or to the ones, I *do* ask. And if I do not ask any? Then, I should say, they will not refer to anything at all. After all, the last exhibits amount to

Ask any doctor(-s) and *the one(-s) you ask* will tell you . . .  
Thus it is  $I$ , who has to supply the reference. To say

Any doctor will tell you . . .

is to issue a *blank* warranty for conditional predictions: you fill in the names. You choose Dr. Jones; well, then *he* will tell you if you ask him. You pick 25 others; then, I say, *they* will tell you if you consult them. . . . If you do not ask any? In this case you do not use the blank; but it may be still good.

To sum up: in saying what he said the sponsor did not make a statement, which could be true or false. He did not make a prediction either, which could be correct or incorrect. What he

did was to issue a blank warranty for conditional predictions, which may be reliable or not; he made a claim, which can be confirmed or disconfirmed, borne out or not borne out. In a sense, he offered a challenge to us, which we may take up or not. Much the same way as "Take any" is not an order, which is obeyed or disobeyed, but an offer, which is accepted or declined.

Having thus put myself way out on a limb, I may expect the objection. "This is sheer sophistry; what the sponsor said is true if every doctor agrees that Stopsneeze helps, otherwise it is false."

You are unfair to our sponsor—I reply—for he did not claim that. Moreover, honest as he is, he would not make a claim that cannot be substantiated. For one thing, who can be sure that there never will be a doctor who would think otherwise? His claim, however, can be substantiated: you may select the doctor you can trust, you may consult as many as you please, and if none of these disagrees, then you may conclude that the sponsor's claim holds water.

"You are lax—you insist—I should not quit till I have asked every doctor in the world."

Now you are not only unfair but illogical as well. The sponsor, in effect, challenged you to *select* any doctor, or doctors, ask them and they will tell you. . . . And now you want to select *all* of them, which, of course, is more than impolite. Remember the apples I said "Take any". Do you want to suggest that short of taking all you did not accept the offer? No, I say, taking all would be an abuse of it. Your requirement of completeness clashes, once more, with the freedom of choice of "any".

"But then,—you ask—what would amount to a confirmation of the sponsor's claim according to you?"

As I just said: I should ask a good many doctors, very conscientious specialists among them, and if their verdict is uniformly favourable (or, perhaps, *almost* uniformly favourable), then I should conclude that the sponsor's claim is confirmed, otherwise not. In other words, I should exercise my freedom of choice, granted to me by the use of "any", in selecting doctors I trust; I should take advantage of the indifference of size of the same particle in consulting as many as I please; but, finally, I should not feel obliged to run through the space-time universe in an interminable search for all the doctors in it, thanks to the incompleteness entailed by the very same particle. If you prefer to call this an *incomplete* verification, be happy with it; I only



remind you that the idea of a complete verification is repugnant to an "any"-proposition.

We noted above that the claim :

Any doctor will tell you that Stopsneeze helps.

is by no means discredited by the fact that no doctor, *de facto*, tells me that Stopsneeze helps, simply because I do not ask any. In a similar way the by-law :

Anybody trespassing on the premises will be prosecuted. may remain in force even if no one ever enters the premises. Such a lack of "existential import" is not limited to rules, regulations, by-laws, or propositions formulated in the future tense. Compare :

Every one of my friends smokes a pipe.

Anybody who is my friend smokes a pipe.

The first proposition would be senseless if I had no friends. Not so the second. It means that if somebody is my friend, he smokes and a pipe, if he does not, he is not my friend, and I do not make exceptions, no matter who be the person. Then it is quite possible that I have no friends. It is not even surprising.

Consider, too, propositions of the sort :

Any nation that conquers the moon can control the earth.

Any perpetual-motion engine would violate the laws of thermodynamics, which is impossible.

It is obvious that we may accept these propositions even though we know that no nation has conquered yet the moon, or, in the second case, even though we imply that there never will be a perpetual-motion engine. After all, one might say things like.

Anybody who could do that would perform a miracle.

Even if one does not believe in miracles.

Thus, in terms of the previous analogy, the blank warranty issued for conditional predictions, or, for that matter, conditional statements, may contain such specifications in the antecedent that nothing actually does or nothing can qualify for it, nothing does or nothing can fill the bill. But then, one may ask, what is the point of making such an empty claim ; what is the use of a cheque that cannot be cashed ? Empty as it is, I reply, such a bill is not given in vain. On the strength of the first proposition one may arrive at the sobering conclusion :

If Russia were to conquer the moon, she could control the earth.

and, on the basis of the second, one may rebut the would-be inventor :

If your contraption were a perpetual-motion engine, it would violate the laws of thermodynamics, which is impossible.

In this second case we argue exactly from the impossibility of the consequent to that of the antecedent.

The importance, therefore, of an "any"-proposition does not consist in the actual fulfilment of the conditions mentioned in the antecedent and the consequent, but in the very relation of these conditions. Such a proposition amounts to the claim that any object fulfilling the condition specified by the antecedent is subject to the condition spelled out by the consequent: if a thing satisfies the former, it will satisfy the latter too; or, at least, if a thing were subject to the first, it would be subject to the second as well. Once more, the "any"-proposition is an unrestricted warranty for conditional statements or forecasts and, we may add, for contrary-to-fact conditionals. In other words, to draw an obvious conclusion, it is an open hypothetical, a law-like assertion.<sup>1</sup>

In discussing the differences between "all", "every" and "each", at the beginning of our investigations, the examples were selected in such a way that the range of the quantifying particle was clearly restricted to a definite and finite set of objects. a group of blocks, a basketful of apples, etc. Later on, while considering "any", we encountered examples of a different sort: they mentioned doctors, trespassers, perpetual-motion engines, and so on, in a rather indefinite way; to put it roughly. we did not really refer to a set of such individuals, but we focussed our attention on the condition of being a doctor, a trespasser or what not, and on the consequences of fulfilling that condition for no matter what individual. Accordingly, the proposition did not presume to identify the candidates; in this respect it remained indefinite and open. To use a simile: we were not interested in the fish caught in the net, but in the net that might catch certain fish; and we were not disturbed if, in fact, it did not catch any.

This shift in the nature of our examples gives us a hint towards a sharper formulation of a distinction that sets "any" wide apart from "each" and "every", and splits the use of "all" right in the middle.

<sup>1</sup> G. Ryle recognizes the connection between the use of "any" and law-likeness. *The Concept of Mind*, New York, Barnes and Noble, 1949, pp 120 ff

We begin by considering two types of sentence we neglected hitherto: questions and negations. It is quite clear that the questions:

Did you see all the pigs in the pen?

Did you see every pig in the pen?

presuppose that there were pigs in the pen. Much the same way the negatives:

I did not see all the pigs in the pen.

I did not see every pig in the pen.

imply that there were some pigs in the pen the speaker did not see, and strongly suggest that he has seen some. The same point holds, in suitable contexts, for "each" too: the question and answer.

Did you reply to each letter?

I did not reply to each letter.

would be out of place if no letters had been received. "Any", on the other hand, does not indicate existential import:

Did you see any pigs in the pen?

I did not see any pigs in the pen.

do not require any pigs in the pen. Moreover, explicit questions of existence, like

Are there any pigs in the pen?

take full advantage of the existential neutrality of "any".

The same point is reinforced by considering affirmative contexts.

Each (every) message you sent was intercepted.

is a correct sentence, but

Each (every) message you might have sent would have been intercepted.

is certainly not. "Any" works the opposite way.

Any messages you might have sent would have been intercepted.

is the correct sentence, and

Any messages you sent were intercepted.

is the incorrect one. Thus, again, "each" and "every" are at home in existential contexts, while "any" sits pretty in non-existential ones. "All", in this case, has a surprise in store.

All the messages you sent were intercepted.

All messages you might have sent would have been intercepted.

are both acceptable in spite of the obvious lack of existential import in the second proposition.

We have to say, therefore, that while "each" and "every" always connote existence, "all", by itself, does not. It may occur, however, as we have seen in earlier examples, in propositions that do have existential import due to some other referential device which may be joined to "all" even within the same noun-phrase (e.g. : definite article, demonstrative-, personal-, possessive pronoun, etc.). This last possibility is not available with "any": we do not have "any the . . .", "any my . . .", etc. We have to say, e.g. "any one of the . . .", i.e. we put the definite article into a separate noun-phrase which then will carry existential import.

"Any" and "all", then, share a common feature: they may occur in constructions lacking definite reference and existential import and, we may add, in this case they occur in the same sense. For, to quote a few examples, in

All messages you might have sent . . .

Any messages you might have sent . . .

or

Try to do it by all means.

Try to do it by any means.

or

All violations will be prosecuted.

Any violation will be prosecuted.

"all" and "any" may indeed be said to be but stylistic variants. And, naturally, none of these occurrences can be supplanted by "each" or "every" without producing a somewhat odd specimen.

Thus "any" and "all" are related and "each" and "every" are related. This is beautifully brought out by the fact that we have two combined forms: "any and all" and "each and every". It is rewarding to look at them for a moment. Consider

Each and every letter has been returned.

Any and all letters will be returned.

We feel that "every" and "all" merely serve here to add an emphasis to the appropriate universality of "each" and "any". But then, of course, the import of such non-existential "all" cannot be different from that of "any", as the import of "every" is basically the same as that of "each". Remember, "every" is "ever each". Is such an "all", then, something like "ever any"? This we do not have, but we have "whatever" (and its kin: "whenever", "wherever", etc.). A little reflection

shows that the latter, again, is related to "any", rather than to "each" or "every".

As we realize that the most common cases in which "all" is used without any additional referential apparatus within the same noun-phrase are formulations of laws, the affinity between "all" and "any" emerges as a point of considerable logical importance. It would take us too far to exploit this idea in detail. Nevertheless, on the basis of our analysis of the use of "any", it is clear that such a study could cast a new light on problems like the recognition of law-like propositions, their existential import and their confirmation.

However this may be, our results are sufficient to show that a simple application of the theory of quantification may fall short of capturing all the logically relevant features involved in the vernacular use of the particles of quantification. Some such features can be found already by contrasting "all" with "each" and "every", but the most important points missed by the theory are the ones that emerge in connection with "any". For we have reasons to hope that a close analysis of this last particle, together perhaps with corresponding logical models, might open up a new line of attack on the problem of law-like propositions. And in these matters a hope is an achievement.

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## II.—THE AUTONOMY OF PRUDENCE

BY A. PHILLIPS GRIFFITHS AND R. S. PETERS

### 1. *Introduction : The Problem of Autonomy*

KANT held that an ethical theory which is compatible with the common notion of morality must exhibit it as autonomous, objective, and practical. If we interpret this demand widely enough to accommodate contemporary manners of speaking, it is possible to look at some recent developments in moral philosophy in Britain as varying responses to it.

Interpreted widely, the demand for the autonomy of morals at least requires that no moral judgment can be deduced from any set of premisses which does not itself contain a moral judgment or principle. It may be that some non-moral facts are necessary to the truth of a moral judgment, but no such set of non-moral facts could ever be sufficient to establish a moral judgment. To say that moral judgments are objective is to say that they are true or false, and that, when true, they are facts independent of the opinions or attitudes of any person or group of persons who may utter them. To say that moral judgments are practical is to say that they are part of a form of discourse which cannot be understood unless it has the function of determining action, or the will. Without this assumption, they are unintelligible.

Intuitionists like Prichard and Ross insisted on the autonomy and objectivity of morality. But there is a problem inherent in every attempt to satisfy both these demands. It is this: how can we regard moral principles and judgments as objectively true, when the principle of autonomy implies that no considerations outside morality could ever be sufficient to determine their truth? This problem led to a comparison between moral principles and other kinds of principle or judgment which could be thought of as true without recourse to inference. The two major types were mathematical principles, which are not inferred from empirical observations, and perceptual judgments which are not strictly speaking inferred at all. The intuitionists therefore suggested that, since no more than the contemplation of a physical object is required for making a correct perceptual judgment, no more than the contemplation of an act is required for perceiving its rightness.

Criticisms of this view have generally depended on developments in logic, epistemology, and metaphysics. But perhaps

one of the major criticisms from within ethics has been that the intuitionists failed to account for the third of Kant's demands, that ethics should be practical. This was pointed out years ago by H. D. Lewis ("Moral Freedom in Recent Ethics", *Proc. Arist. Soc.* vol. xlvii. 1947-48) who, though sympathetic to the intuitionists in other ways, showed that in the work of most intuitionists the problem of how ethics can be practical was ignored or not fully faced. Nowell-Smith and Hare have more recently concentrated on this issue. They insist that the intuitionist thesis leaves no room for an explanation of how or why anyone should take any notice of these ethical truths which present themselves to the intellect. Taking the issue of practicality seriously, they cannot be content with the answer that it just happens to be the case that human beings do take notice of these truths. For this makes the practical character of morality only accidental, whereas the demand is that we should see morality as essentially practical, and as unintelligible otherwise.

From this demand follows the attempt to understand moral discourse in terms of other kinds of discourse which are obviously and essentially practical. Moral judgments, it is argued, are intimately connected with action, so they must be similar either to expressions of the causes of action, emotions or emotive dispositions, or to the signals of action, such as decisions, announcements of intention, and commands. In this way the practicality of morality is stressed while preserving its autonomy. For neither expressions of emotion on the one hand, nor announcements of intention or commands on the other, are logically deducible from any set of indicative statements of fact. If they are deductions at all, they are deducible only from other decisions or expressions of emotion. Strictly speaking, they are not deducible and do not follow from anything; for expressions of emotion and announcements of intention cannot significantly be said to be either true or false. What we have here is practicality and autonomy at the cost of objectivity in ethics. This position is perhaps not only compatible with, but demanded by the metaphysical dogmas of the recent past. Nevertheless it presents a greater paradox to common sense than intuitionism.

In the face of these difficulties there is an almost inevitable third possibility: to save the objectivity and practicality of morality at the cost of autonomy. Some philosophers have recently made this move. Their arguments lead to the conclusion that morality is intelligible as both objective and practical only if it is possible to regard it as required by prudence. Many of course might feel that the moral views they hold could never be

justified in this way. This is, however, another issue which we do not propose to discuss. In any case, it does not matter for the purposes of this paper. For our thesis is that there are good grounds for saying that judgments of prudence are themselves autonomous. So a view that denies the autonomy of morals by showing that it rests on prudence ends up by being saddled with autonomy in another place—that of prudence. Its effort to get rid of autonomy in the field of morality, however successful, can prove at best to be a hollow victory.

We shall argue, then, that many of the judgments which we might be inclined to call prudential—judgments about what a man's true interests are, about what gives true satisfaction, or what is someone's good—are also autonomous. Such judgments are quite distinct from judgments about means to ends, which may be called judgments of technical prudence and require no more than a knowledge of the world for their truth, as Kant pointed out. As such, they are clearly not autonomous. But of course they have no practical relevance until they are connected with some given end. If this end is one determined by the rational consideration of what is for the agent's benefit, or what concerns his own real interest or true good, then it is determined by a judgment which is prudential in a second sense, which we shall call private prudence. Such judgments of private prudence are not judgments about how to get what one already proposes to get, but about what one should propose to get in so far as one is considering one's own interests rather than the interests of others. Private prudence will be useless, as indeed any sphere of reasoning about ends is useless, without proper attention to technical prudence in deciding on means; but without it or without some sphere of reasoning like it, such as morality, in which ends are considered and determined, technical prudence itself becomes pointless.

## 2. *Prudence in relation to wants*

Mr. A. C. MacIntyre has suggested a position which would conform to the third possibility outlined above (in "Hume on 'Is' and 'Ought'," *Philosophical Review*, October 1959, and in Two Third Programme talks on the same topic). It may be put briefly as follows. This argument is a valid one:

If I knife someone I will get jailed  
I don't want to get jailed  
Therefore I ought not to knife anyone.



In it, a practical conclusion is inferred from factual premisses. Therefore there are some practical judgments which are derivable from matters of fact without further reason. These are possible because one premiss concerns what is wanted; and in general, the only valid arguments which lead to practical conclusions are of this kind, involving some such concept as wanting, need, or desire. All other arguments, including those deductively established from some so-called fundamental value-principle, are unintelligible. Moral judgments, and, of course, prudential judgments, can be derived from and can only be understood as dependent on facts about what people want.

MacIntyre does not want to claim that the argument quoted above is *deductively* valid: he does not claim that its premisses entail its conclusion. He claims only that it is a good argument. It may therefore seem that on the definition we have given of autonomy, there is no difference between his position and ours. The important point is, however, this. Those who have insisted on the autonomy of ethics have appeared to do so sometimes because they have analysed ethical judgments in a certain way; and the discussion has consequently centred round such issues as what is meant by 'validity', whether moral principles are major premisses or principles of inference, whether all acceptable arguments must be capable of being put in a deductive form; or else, less recently, on whether goodness is simple or complex, natural or non-natural. But what seems to have been behind their insistence on the autonomy of ethics was a common dislike of a certain philosophical move, the kind of move Moore thought could never be made again once he had 'exposed' naturalism: the move which goes "x is good, because that is the very meaning of the word 'good'". It is not enough, from this point of view, to describe the way we actually argue, even if there is one common way; it is necessary to justify it. The talk about unbridgeable gaps in deductive arguments, about missing major premisses, or the necessity for the intuition of non-natural qualities, comes about because room must be left for the question of justification to be raised. MacIntyre is arguing that there is no need to speak of missing premisses, of unbridgeable gaps, or mysterious intuitions; for there is only one way in which we either do or can back our moral judgments: by appeal to what is wanted, needed, or desired. The question of justification need be raised no further, because only an appeal to what is wanted can make moral judgment and moral action intelligible.

Now it does not matter whether we regard MacIntyre as denying the existence of Moore's real problem or propounding its

solution: what matters is whether his view of the necessary character of ethical reasoning is correct.

The grounds for his view seem to be these. First, human action is intelligible as such only so far as what is done can be connected with something the agent wanted or desired. Hence, acting in accordance with a moral or prudential rule is intelligible, as action, only under these conditions, and must otherwise be represented as some kind of nervous tic or irrational obsession. Secondly, it follows from this that any form of language that has the function of guiding action can have point only if by its use it is possible to influence and persuade people. All action is connected with the wants of the agent; hence no action can arise from attempted persuasion which is not connected with the agent's wants. What a man can do is limited by what he wants, and what he ought to do by what he can do. Thus moral and prudential rules are intelligible as rules which people follow, and which can be justified, only if they concern what is wanted.

The view that human action is intelligible only in so far as it is connected with what is wanted is false if it means that all human action must be explicable in terms of the feelings of desire, cravings, and the like, of the agent. Much of our conscious purposive action cannot be explained in this way, though it is not inexplicable. The view is true only if it means that human action is purposive, that it involves the conception of an end. Where this is the case then quite clearly and necessarily, if somewhat trivially, what is done must either be done for the sake of something or it must be the end for the sake of which something is done. But this is only because 'what is wanted by the agent' means no more than 'what the agent's end actually was'. In this sense of 'want' it is quite misleading to speak of what a man wants as always *explaining* what a man does. It may of course explain what he did as a means to his end; but it may, on the other hand, only point out what his end was, which explains nothing, rather, it only describes what sort of action it was. Or perhaps, as when we say 'he kept the promise simply because he wanted to' it puts what he did solidly within the class of human actions and at the same time denies that doing this action was a means to any *further* end, like getting social approval. Thus the truth in this doctrine amounts to no more than the truism that human action involves the concept of an end. The conclusion drawn from it, that practical principles are unintelligible unless action in accordance with them connects with something the agent wants, amounts to no more than the unilluminating

observation that action in accordance with a practical principle is unintelligible unless either it is the agent's end to act in accordance with it or he has some other end which involves action in accordance with the principle in question. It does not follow therefore that he must want what is prudent because he already wants something else.

Now what of the conclusion that what a man can do is limited by what he actually wants? This can only help MacIntyre's case if it means that the ends which a man *can* have are limited by the ends he actually *does* have. This is a very odd view in itself, and again does not follow from the fact that what a man does must be connected with what he wants, where what he wants is equivalent to what his ends are. From this it follows that a man's ends in acting are limited to what he wants only because the ends a man has are the ends a man has. But what a man actually wants does not limit what he can want, because what his ends are is not the same as what they may become. If prudence or morality recommends ends distinct from those which a man actually has, all that is necessary is that the other ends recommended are possible ones for him. The persuasive or action-guiding value of these spheres of reasoning are not affected by this. A man may simply want to be prudent, or to appear moral. There is no *a priori* reason why we should deny that people can listen to reason and want to do—that is, have the end of doing—whatever anyone can convince them is the most reasonable thing to do. Presumably people who seriously seek advice put themselves in just this position. Moreover, before people get to this highly civilized stage of listening to reason, they respond to authority. They may not in such cases even want to obey the authority. they may not be conscious of it. But they may acquire rules which in a later and more critical state of mind they reflect on and try to justify. Again, one may take advice, or adopt some policy, for quite extraneous reasons—such as to gain the respect of the person who gives it, or to gain some social approval. What is at first adopted for such reasons may soon become an end in itself, which shows sometimes how valuable, and sometimes how dangerous, snobbery can be. People usually take up smoking as a social affectation, but it may soon become a vice. It is still nevertheless true that a lot of people cannot be touched even by snobbery, or worse that their own snobbery is of a corrupt and degraded kind. But even in such cases, where we are confronted with people who are just unpersuadable, the making of prudential judgments with regard to their interests is not pointless. One may be in a position to force people to do certain things and, in

such a position, one may have a moral obligation to decide what is best for them—for example, if the person concerned is about three years old.

It does not therefore seem that facts about what someone wants are the only possible or intelligible grounds for moral or prudential judgments. But could not the position under discussion be shown to be even more untenable? For *could* the facts about what someone wants ever be sufficient to establish, not only what he ought to do, but what is best for him ; that is, what is demanded by private prudence?

It is obvious enough that what a man says he wants is not the same thing as what he really or actually wants. Even supposing, as Kant wrongly thought, that a man's desires are known to him through inner sense, it is clear that a man can seem to himself to want one thing, and really want another. So when we speak of what a man wants we normally mean his actual rather than his avowed objectives. However, MacIntyre (in his broadcast talks) speaks of what a man *really* wants, not as either his avowed or his actual objectives, but as what would satisfy him in the long run rather than what he from time to time tries to get. This presumably means that what a man really wants is what he tries to get and, getting, tries to keep. But even in this narrow sense of 'want' considering only what a person wanted would hardly seem sufficient for our everyday judgments about what is good for him. For by these criteria we might be forced to say of someone that he wants nothing but power, and be unwilling to qualify this by saying that what he really wants is something else. He is certainly satisfied with what he has got, except perhaps that he would like more of the same. But because his life is narrow and devoid of all valuable human relationships, because he is culturally and intellectually dead, we might still want to say he is a miserable or an unhappy man. But this does not mean that he is foolish or careless or rash, which would be to diagnose his plight as due to a failure of technical prudence ; nor that he is discontented ; nor that he is unhappy in Kant's sense, for he may be successful in everything that he wants or wills. Such a man does not suffer from unsatisfied desires ; his trouble is that he finds satisfaction in what is really unsatisfactory. What appals us about the life of such a man is not that he is not getting what he wants, but that he wants just what he is getting. Such judgments of private prudence, it is clear, go beyond anything the agent seems to himself to want, or actually does want, and concern what he ought to want for himself, and what a wiser person would find satisfaction in.

### 3. *Prudence and Human Nature*

So far we have considered only the question of what facts about an individual's wants are sufficient for determining what is in accordance with private prudence. But doing so obscures an important point. For what the individual wants is not, as we have presented it, a simple matter of observable fact. We would not be prepared to count as an end simply anything which any individual seems to try to get and to keep. A man who spent his time putting either a small triangle or a small circle on a pair of size ten boots on every wall over ten feet high, unless it was a Tuesday, could hardly be described as having the over-riding end of wanting this without some special explanation; we should be more likely to call him mad, or, if it is some derisive surrealist protest, one who simulates madness. What counts as evidence for wanting something is more than the actual behaviour involved. It makes sense to ascribe wants to a being only if we understand wanting in terms of the full-blown case of the man who deliberates and chooses, who considers what he should do. His prudential deliberation and intelligent choice are intelligible only if he is capable of having wants, but his having wants is fully intelligible only if he can regulate and criticize them. (The importance of this will be brought out towards the end of section 4.) Furthermore, what counts as evidence for wanting something depends on what we are prepared to count as a possible end without special explanation; and what we assign to this class of possible ends will depend on considerations which go beyond our observations of the individual concerned. It will depend in part on the way we think that human nature limits the things a man could reasonably be said to want.

Now there is an obvious sense in which we must assume a common human nature. Men deliberate and choose; they are not just drawn towards goals like moths towards a light. They plan and impose schedules on their wants. They distinguish between what they ought to do and what they want to do. But this account of human nature is purely formal in that it merely articulates the set of concepts which are necessary to describe what any man may be doing. But the *content* of this scheme is filled in by reference to the standards of particular societies. That which a man wants, that for the sake of which he acts, his end, is something which has been picked out and named as a result of a social life which has reached the level of describing, justifying, and explaining what a man does. Such 'ends' or 'objects of desire' are correlative with 'means'. And 'means', like 'ends' have standards built into them, standards of social

appropriateness as well as of efficiency. For a man can seek promotion honestly and ruthlessly as well as doggedly and painstakingly. We explain and justify largely in terms of the framework which we, in our particular societies, have ourselves imposed on the plasticity of infantile responses. We look on an action as *prima facie* unintelligible either if it is something that seems to have no point at all or if we cannot accept the point ascribed to it by its author or if it seems a grossly inefficient or socially inappropriate way of achieving its alleged objective. So actions which require special explanation in some societies will be taken for granted in others—*e.g.* keeping or erecting monoliths.

As a matter of fact, if a case is to be made for a common human nature which does not depend on a common social heritage, this would have to be made at the level of the bare wish, rather than at the norm-ridden level of wants. For 'wish' is not connected in the same way as 'want' is with the notion of means to an end. We can wish for things without any idea of how to get them, like the moon or to be invisible. Indeed we can only really *understand* what a wish is by withdrawing some of the criteria which are built into the concept of 'want'. Freud brought this out very well when he maintained that the primary processes of thought are characterized by wishes which are untroubled by a sense of time, of cause and effect, of logical connection. Perhaps in such vague, unruly, undifferentiated urges is something that might be called a common nature; but we would hesitate to call it *human* nature until such wishes become attached to socially approved and selected objects and until canons of logical relevance and causal connection begin to be imposed on this autistic amalgam. Indeed a proper understanding of 'wants' as distinct from 'wishes', and their connection with teachable standards of what it is *reasonable* to reach out and clutch, does much to throw light on psychological theories of human nature, such as Freud's, which have often been thought of as providing, as it were, a ground-plan of human nature to which a naturalist might appeal in justifying counsels of prudence. Such theories are to be seen largely as giving special explanations of deviant wants rather than as revealing the reality beneath the appearances of standard wants.

The Freudian account of the miser, for instance, does not deny that he wants money; but as he wants something in a way which, according to our standards of appropriateness and instrumentality, is pretty absurd, a special explanation of why he wants only money is offered. This is in terms of wishes for things like power and organ pleasure which persist because of the manner in which they

were dealt with in infancy. No such special explanation would be required if the man were a business man who was just reasonably cautious because of his estimate of the trend of the market. Freud gave special explanations for the behaviour of misers, homosexuals, and perverts. In so doing he surely assumed a standard development of wishes into those wants which were thought appropriate in the society in which he lived—for food, sex, power, and friends. He assumed a norm of development towards what Abrahams later called 'the genital character'. If men do not, for special reasons, get stuck at different stages in their development, they emerge with a more or less standard equipment of wants together with a realistic appraisal of causal connections. They are lucky enough not to be troubled unduly by the unfulfilled and rejected wishes of their unconscious. Indeed many writers have attacked Freud because his prudential policy encourages conformity with the existing standards of society.

Our arguments so far have shown that explanations and justifications in terms of what is wanted presuppose not only a general conceptual framework but also a concrete filling for this framework which is provided by the particular society in which a man lives. But, it might be argued, *must* counsels of private prudence be as culture-bound as this? Is there nothing in terms of the contents of human nature which might provide judgments which escape the local autonomy of differing cultural standards? Could not, for instance, the followers of Freud, Fromm, and Horney argue that men have two basic instincts, sex and self-preservation, which are manifest in the basic needs for love and safety? These are to be regarded as basic in the sense that they must be satisfied if a man is to remain both alive and viable—if he is to hold down a job, get on with his fellows, and do all the other things which he wants to do. Of course he may not manage to satisfy them all adequately. In this case he will be either dead or in a mental home. He may, however, make a partial job of it, like a half-starved man. In this case he will be more or less viable, but a neurotic beset by unconscious conflicts—like a miser. He manages. But his style of life is witness to the need for safety being satisfied in a way which disregards the need for love and to love. If someone says "But he's doing all right, isn't he?" the Freudian would simply say "No". Imaginary misers are not to the point, he would say. Examine any such miser and you will find him beset by conflicts and frustrations. For if these basic needs are bungled or thwarted, there will be characteristic troubles which extend over a whole

range of wants. Is the mild paranoid doing all right when his estimation of the situation in which he must act is constantly distorted by the all-intrusive thought that people are plotting against him? A man can only do most of the things which he wants to do if these basic needs are not grossly thwarted. The evidence for this is to be found in mental hospitals and in seedy bed-sitting rooms in Hampstead.

Now if such a doctrine is true it has important consequences for any doctrine of prudence. But it is very important to get clear about the way in which it is relevant. Have we here a case of a theory of human nature generating at least *some* counsels of private prudence? It is the more plausible to say so because this doctrine does not seem to be telling us what technical prudence usually does, namely what is necessary to satisfying particular wants. But there is surely the alternative that this doctrine concerns what is necessary for satisfying *any* system of wants: that it establishes what might be, as a matter of empirical fact, the most general procedural principles of technical prudence.

It might be argued that Mrs. Foot has shown something of the sort in relation to the concept of 'injury'. (See P. Foot, "Moral Beliefs", *Proc. Arist. Soc.*, 1958-59.) She argues that every man needs the proper use of limbs, because as a matter of fact without limbs a man cannot do what he wants, whatever he wants. So far as we want to do anything, we must try to avoid putting ourselves in a position where we are likely to be injured. There is at least one judgment, therefore, which must be accepted and which follows from sheer matters of fact about the human condition: one ought to try to avoid injury to oneself.

Now in general when we speak of what someone needs, the judgments which we make are not quite like this. The concept of 'need' is, it is true, necessarily connected with the concept of injury: a person's needs are for those things the lack of which is injurious to him. But the standard of what constitutes injury may be derived either from a man's function, as when we talk of a carpenter's need of a saw, or from some particular want, as when we speak of a man's need for money in order to buy his dinner, or from some end which all men are presumed to have as a matter of fact, as when we speak of a man's need for oxygen in order to keep him alive. Mrs. Foot's example is however quite different from these. It relates neither to specific wants nor to just being alive, but to any wants whose satisfaction constitutes being alive in some manner. The manner would vary according to personal preference and social standards. Her example is not of a judgment within private prudence; for it stipulates no specific ends



which a man wants. It is technical in relation to any system of wants in the way in which ordinary judgments of technical prudence are technical in relation to particular wants.

Now judgments such as those generated by the Freudian theory of human nature, like 'a man needs not only food, water, and oxygen, but also love and security', are not "technical" quite in the way in which Mrs. Foot's judgment about the use of limbs is technical. For these needs are not just for things like the use of limbs, but for objects and states of affairs which we use our limbs to obtain and bring about. Also they do not, like the judgment that every man needs the use of his limbs, state a necessary condition for the satisfaction of any particular want, rather they state necessary conditions for the effective *regulation* of wants within a system, whatever these wants may be—for what is sometimes called the integration of the personality, or the harmony of the soul. These differences, however, do not make such judgments of what a man needs any less technical than those mentioned by Mrs. Foot, though they may be more fecund. For the question still remains about which wants are worth satisfying whatever story the psychologist may tell about the necessary conditions for regulating these wants in relation to each other, for avoiding unconscious conflicts, being adjusted to reality, or achieving mental health.

A Freudian, of course, might want to make a case for activities like eating, drinking, and sex of a rather different sort. He might claim that these types of activity are not just concerned with what a man needs in the sense in which we have tried to exhibit; they are also activities which are 'natural' in a sense in which art and arithmetic are not. For what we call 'civilization' or culture is an imposition of rules and restrictions on a small number of basic wants which take a bit of time and a standard environment to mature out of undifferentiated wishes, together with devices which we adopt to deal with such impositions. Sexual intercourse is a 'natural' activity, but it takes time and a favourable environment to mature. While it is developing through its various stages restrictions and rules are imposed on its embryonic manifestations. If these rules are taken into the mind of the growing child, we get the emphasis on tidiness, regularity, and order which is so characteristic of civilization; but another way of reacting to such threats is to sublimate the wishes into wants that have displaced objects. And so the plastic arts develop as sublimations of a primitive component of the sex instinct. Wants, in other words, can be arrayed in tiers, with the bottom level nearest to 'nature'.

This strange explanatory story, whatever truth it may contain, certainly has the function, like the Marxist's story, of influencing judgments of prudence, as we shall later show. But it really is of little help—even to a Freudian. For on this sort of view scientific investigation itself, to which any Freudian is committed, is an 'unnatural' activity—a sublimation of infantile sexual curiosity or a method of reassuring himself against insecurity. Nevertheless he himself has to make prudential judgments in which he has to weigh the amount of time which he is to give to science against the rival attractions of food, sex, and the pursuit of power. The explanation which he gives of these activities may influence his judgment. A Freudian, for instance, would be unlikely to discount the claims of sex in the way in which an artist or a religious man might. But such a view of human nature would do little more than limit the range of his prudential judgments.

In this respect a man like a Freudian, who has a very special theory of human nature, is in a similar position to that of most people; but his judgments tend to be biased in one kind of direction. For, generally speaking, these sorts of psychological considerations do little more than limit the range of the practice of private prudence. For a man might have plenty of food, sex, drink, and security—a pastoral life surrounded by the delights of the farmyard. But when we spoke to him of the delicacies of human relationships, of art, of the excitement of discovery, he might spit and say that a man who bothered about all that needed his brain testing. We might, of course, be indignant at his contempt for what other people found satisfying, at his lack of respect for others who did not share his values. But both we and Freud might also find his style of life deplorable in itself, we might think, perhaps, that such a man was stunted, that, although intelligent, he had shut his eyes in an *arbitrary* way to the things which he might want. We might condemn him because he fails to want what he should want, not because he fails to get what he does want. Facts about what he wants would be what we pass judgment on, not simply the grounds of our judgment, and if in making such prudential judgments we have to take into account a shared human nature, such considerations take us no great distance in condemning such a man and his ways. Our judgments in this case are autonomous ones, and they are, of course, notoriously difficult to make and to justify. Perhaps there can be fundamental prudential disagreements, and they are, ultimately, impossible to settle. In view of this, we might ask: why bother at all? Is it necessary to make such judgments?

4. *The necessity of private prudence*<sup>1</sup>

If the question whether it is necessary to make judgments of autonomous private prudence is the same as the question 'why be prudent?' then the short answer to it is that the question itself is one of private prudence, and would not be asked unless the asking and the necessity for answering prudential questions were not already presupposed. But a man might refuse to ask even *this* question—a man might practise indifference to private prudence without contradicting himself by trying to preach it. And it is clear that it would be impossible to give reasons to a man who knows only what Plato called 'the necessary appetites' for indulging in any activities beyond them. We could say nothing to him. But perhaps we can say something about him.

We might say about him what the Stoics said about such people: that they were in a state of ignorance and illusion. To the Stoics the passions were false judgments. Thus Chrysippus said that avarice is a supposition that money is good. Pleasure is the gratification of passions, and as such is irrational elation, whereas joy is rational elation. (Diogenes Laertius, vii. 110-117.) Now of course it would be difficult for us to accept these views as they stand. Their emphasis on the passions as false or degenerate judgments nevertheless has point. Both Kant and Hume, and their successors among empiricists, have regarded desires as matters of brute fact, given either to introspection or through the empirical observation of behaviour. The question to which they gave opposed answers is whether all practical judgment—though it was usually morality that came into question—can be understood entirely in terms of desire or whether it requires some kind of leap into a different 'rational' realm. The Stoics on the other hand started at the other end of this disputed chain. They took as their standard action in accordance with reason aiming at a level of fittingness which they called nature. The emotions, as false judgments, were seen as failures to meet this standard. To see men as essentially rational is to see anything which is not the outcome of a critical attitude which recognizes the need for justification and tries to meet it, as a dropping away from the norm. It is to say that wants are not fully intelligible as they stand; they can only be understood as attempts to formulate rational ends. It is not that only a rational being can have wants: we attribute them to animals. It is rather that wanting can only be properly understood in terms of the

<sup>1</sup> We are grateful to Professor Ryle for comments in the light of which we have considerably emended this and the following section.

full-blown case of a being who deliberates and chooses among ends by a consideration of their nature. To want something involves conceiving it in a certain way, and as different from something else, and this immediately brings with it the possibility of the question why *this* rather than *that* should be wanted.

On the other hand it is certainly true that it makes little sense to speak of private prudence with regard to a being to whom such concepts including the concept of wanting, do not apply. There is therefore a sense in which the concepts of what is best for a man and what he ought to do are unintelligible unless related to the concept of wanting. But the relation is not one which allows us to say that what a man ought to do is justified in terms of what he wants; rather it only makes sense to speak of what a man ought to do if he is capable of having and critically regulating wants of his own.

The a-prudential man is not therefore someone who holds a position complete in itself from which he can reasonably ignore what he ought to want. His having wants at all is fully intelligible only in so far as we regard him as a rational being who is capable of self-regulation and a choice of ends. We can regard him as failing in the rational completion of activities in which he is already trying to engage.

Whether this view of the organic relation between wants and their critical assessment by the agent could be sustained or not, it does seem clear that the empiricist alternative of Hume and Kant must be abandoned. What is wanted is not a matter of what men reach out and clutch. We can, given the concept of wanting something, detach it from its natural and original application—to a being who is in some degree rational—and treat movement towards and fromwards as practically sufficient evidence of wants. It is in this way that we can attribute wants to animals and even plants. But not all cases of wanting could be like this, for there would be then no way of distinguishing between organic or mechanical movements towards something, and wanting something. So that when one is speaking of *men*, in relation to whom the concept of 'wanting' has its natural home, what is wanted is not a matter of what *men* reach out and clutch. For what is wanted has to be contrasted with what a man merely is moved towards, and not anything which a man appears to try to get can be said without special explanation to be wanted by him.

##### 5. *The possibility of private prudence*

Our case is then that talk of what is wanted drives us on into talk about what ought to be wanted. It could still be objected

that what now seems to be necessary is nevertheless impossible. We have come, of course, to the abyss where Mill handed over to the expert to decide what activities are superior to others, and where Moore could only dogmatize about inspecting states of affairs that are intrinsically good. Their difficulty is also ours. If we ask what the activities which we recommend in our judgments of private prudence have in common, as Spinoza and others have done, we are not altogether at a loss for an answer. We might stress their non-competitiveness, their permanence, their fruitfulness. But to do this would really only be to articulate in more detail what we commend, not to justify our commending it; and both Mill and Moore were equally well capable of doing this. To commend what we commend simply and solely because it is what it is seems far too arbitrary a process to be worth calling a rational consideration of possible wants. It is precisely this consideration that has led such writers as MacIntyre to attempt to see prudence, and, for that matter, morality, as heteronomous.

The very difficulty with which we are faced here may however provide a clue, though at this point not much more than a clue, to what may be said in justification of the activities which private prudence recommends. What we call civilization, and play, consist largely in activities whose standards are non-technical in the sense that they are not rules for bringing about biologically essential ends or for bringing such ends about in a manner which is purely a matter of efficiency. It is no accident that manners are associated with civilization; for when the manner in which ends are pursued begins to matter almost as much as actually achieving them, talk of civilization becomes appropriate. Indeed in a lot of games and other civilized activities 'ends' have to be invented to provide terminating points for the skills displayed. Such activities can be taught; one can be taught to want certain things rather than others. In being taught this one is learning to apply criteria to one's activities which define the activity itself. The activity is not defined by some end outside itself; and in being taught it one is not being taught to achieve something distinct from what one is being taught. One is being taught *what* to do as well as how to do it well. One is thus learning to conceive the activity in a certain way, as well as to do something. Until a man has been taught or has learned something of this kind, nothing can be said to him in advocating other civilized activities; he must be able to imagine what it would be like to do other things for their own sake even though he is ignorant of the *particular* criteria of the

activity in question. It is not so surprising, then, that no justification can be given of such activities which amounts to more than a description of them. For although, in so far as a man is teachable, he will learn to apply the criteria by which the activity is defined, and will understand that other activities are defined in terms of other criteria, he will not be able to make judgments of *comparison* without practical experience of the activities in question. This is presumably the point of Mill's appeal to the expert. But prudential judgments are not, as Mill mistakenly thought, a form of expertise; for the notion of 'expertise' makes sense only *within* an activity or when a skill is regarded as instrumental to an agreed end.

Perhaps these considerations also point to two further lines along which the justification of prudential judgments can be approached. The first is this. If private prudence is necessary, then we must assume its possibility. If it is possible, then the sort of judgment that is involved should not be unconnected with the conditions in which it becomes possible. As autonomous, it is itself a form of activity in which the question "why do this rather than that?" is pressed until it can be pressed no further. A man could not mature in such prudential judgment unless he already knew what is was to engage in activities which supplied their own standards and which were, for him, their own end. If private prudence recommends civilized activities, it is recommending activities which are formally analogous to itself and which must be engaged in if prudential thinking is to be humanly possible. To appreciate the beauty of a picture or the validity of an argument requires a degree of detachment, a lack of involvedness so that the products can be surveyed and assessed; in the same way prudential judgments presuppose a degree of detachment without which an assessment of a man's way of life is not possible. The connection, therefore, between prudential judgments and the advocacy of civilized forms of activity is not a purely contingent connection.

Secondly, if our argument suggests that there are reasons for engaging in one activity rather than in another, it also suggests that these reasons cannot always relate to something beyond these activities. A man who is prepared to be critical about his life must accept reasons internal to the activities which he is assessing. In doing so he will have entered into a difficult and possibly endless problem. For the description of disinterested activities, and hence, the discussion of their value, is not a matter of mere observation. They are in part what human beings make them and in part determined by the material with which they are

engaged in its social and natural setting. What we think of science or history of art as will depend upon our view of the nature of things and of the human mind. It is far from being the case that these activities can be judged by a simple intuition. For our ideas about human nature, as well as cosmological, sociological, psychological and theological views, will affect the way in which we understand them. Marx, for instance, thought of men as tool-using animals concerned only with the satisfaction of basic needs ; this conception led him to conceive other wants—e.g. religious ones, as merely by-products of such fundamental needs and to impose a characteristic stamp upon his prudential assessments just as Freud's did. His assessments appear crude and absurd to a religious man who sees life as a relationship with God and who considers that even eating and drinking should be regarded as a sacrament. Thus some of the considerations to which private prudence will appeal will be in criticism of the conception men may have of their own activities. Perhaps the Stoics were wrong in saying that all desires were false judgments, but perhaps we could say that all rational desires involve necessarily a conception of their end, and it is with the criticism and, perhaps, the adumbration of new conceptions of this kind, that private prudence is primarily concerned.

#### 6. *Prudence and Morality*

What we have called private prudence, it might be claimed, is not really distinct from morality. Our reference in this context to the Stoic conception of virtue suggests this. It might also be argued, for instance, that while Kant does not limit prudence to technical prudence, for private prudence is for him the system of a man's *actual* wants and aims, he allows for what we would call judgments of private prudence but includes them within the sphere of morality (for example : ' One ought not to neglect one's talents ' and ' One ought not to kill oneself ' ). As a mere matter of giving names to kinds of judgment this issue is unimportant. Probably the distinction which can be made between autonomous private prudence and morality is not reflected in the senses of the words ' prudence ' and ' morality ' in English. The distinction is nevertheless important and it is the business of philosophers to introduce such distinctions.

Whatever the ordinary use of the term " morality " may be there is one very general characterization of morality which most philosophers would accept—that it concerns those considerations which are over-riding in determining what ought to be done.

In this sense of "morality" we can speak of a man as having a religious or aesthetic morality. Now prudence, as we understand it, consists in considering what ought to be done in so far as it affects only ourselves, just as benevolence consists in considering what ought to be done in so far as it affects others. Both of these are limiting considerations in relation to morality which concerns what a man ought to do simply, without such qualifications. But, of course, considerations both of prudence and of benevolence can come to be regarded as part of a man's morality if he thinks that he *ought* to consider his own interests or those of others. Indeed these can be hived off and erected into mutually exclusive policies for living, which are usually referred to as egoism and altruism. It is, however, more usual to believe that the interests both of oneself and of others ought to be considered and to insist that some reason must be given for regarding one person's interests as less important than another's. Somebody, too, might hold that there are other fundamental principles, such as that the truth ought to be told, which ought to be adopted for themselves alone, and not for reasons to do with people's interests. But this possible proliferation of principles need detain us no further. For our concern has only been to show that though morality may presuppose either prudence or benevolence, it cannot be identified with either; for whether people's interests ought to be considered is itself a question which must be decided in determining a policy for living.

Defined in this way it is possible to see how private prudence can be autonomous and yet inferior to morality. It is autonomous because the qualification 'in so far as it affects a man' does not constitute what he should do; it only limits it. Morality is superior, or over-riding, because in making a judgment solely from the point of view of private prudence a man is arbitrarily limiting his consideration of what he ought to do. Whether he should or should not so limit his consideration cannot be considered purely from the point of view of prudence; it requires an over-riding judgment.

The distinction is important, then, in determining whether the content of morality is identical with that of private prudence; for while it is possible that it is, for instance, if God had arranged things in the manner suggested by Sidgwick, the distinction we have made shows that it is not logically necessary that it should be so. Thus while morality is, in the sense we have explained, superior to private prudence, it is in another sense dependent on it. For it would be a strange morality that had *no* regard for *anyone's* interests. Even the most die-hard intuitionists have



always held that benevolence is part of our moral duty ; what they have denied is that certain other duties can be shown to be dependent on the consideration of interests. It would therefore be necessary, in deciding what ought to be done, to be able to determine what people's interests are. This is where judgments of prudence come in, since benevolence itself is parasitic on prudence in relation to its content. For granted that we ought to consider others, we must have a notion of whether or not each individual affected by our actions is or is not benefited by them. How else do we tell this except by judgments of prudence which we make in relation to others rather than in relation to ourselves when we consider what is good for them? To know what is prudent is not necessary to justifying morality ; but it is necessary to giving morality content. Morality without prudence is at least half empty ; prudence without morality is at best myopic.

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lightning. These uses connote the making up of his mind, or the state of his mind when made up, especially his refusal to be dissuaded or deterred. One who is, in this sense, determined to do a thing is determined not *as a result of* pressures upon him to do it, but, more likely, *in spite of* pressures on him not to. Similarly, a determined person is not a person who is overcome by his difficulties of physique, temperament or situation, but one who persistently exerts himself to overcome them.

As with (1) above, to say that a person is determined to do something is not to imply that he will inevitably do it. For (a) a man can be said to (be) determine(d) to do something that is not in his power, provided he supposes that it is—*e.g.* square the circle or turn base metal into gold ; and (b) we may say “he is determined to do it if he can”, meaning that he intends to make every effort to do it while recognising that it may prove impossible. In any case he may be prevented from or fail in doing whatever he is determined to do. Moreover, the thesis that all actions are determined clearly fails in this sense of “determined” as well. For we do not invariably or even often act in this state of mind.

(3) A man can be said to determine the height of a pyramid or the date of the next solar eclipse, when he has calculated them from available data. A thing is determined in this sense when the answer to a question has been worked out by inference from what is already known. It is with this use that the usual meaning of “determinable” is connected. For to say that something is determinable is to say that methods and data are available for finding out the answer to a question. A distinction may be noticed here that will become important later. To say that one thing is determinable *from* another is quite different from saying that it is determined *by* that other thing. For while the height of a pyramid is determinable *from* the length of its shadow, its height is certainly not determined *by* the length of its shadow. Rather, the length of its shadow is determined by its height *inter alia*.

This use, unlike those in (1) and (2), has no special connection with matters within our control. For when a person has determined something, in this sense, he has not answered a practical question, but a theoretical or factual one, upon some matter which may, like the date of an eclipse or the value of  $\pi$  to 200 decimal places, be outside his or anyone's control.

The claim that all actions are determined is clearly untenable in this sense as well. For at whatever time it was made, it would presumably mean that all questions concerning the conduct of all agents, past, present and future, had at that time already been

correctly answered. Needless to say, it is not the case that all such questions have been answered. Most of them, indeed, have never even been asked.

(4) An issue can be said to be determined not by a *person* but by one or more impersonal *factors*. For example, it might be said that the number of guests invited to a country wedding was determined by the size of the village hall. In this kind of assertion we isolate a particular factor that entered the calculations of the planners, and ascribe to it the power of a person to decide a practical issue. We imply that the factor was accepted, for current intents and purposes, as unalterable. The hall could not be enlarged, and there was no other available. Against this background we regard its size as the deciding factor, the basic consideration which decided them to invite a specific number of guests.

Here there is no implied limitation upon the agents' freedom either to take or to implement a decision on the matter. They might have refused to be restricted by the hall, and held their wedding in a nearby town. In saying that the size of the hall determined the number of guests we mean only that it was a basis or starting point from which the rest of their planning proceeded. Moreover, it is certainly not the case that all actions are determined in this sense. For it is only reasoned decisions that can properly be associated with determining factors of this kind, and many actions are not the effects of reasoned decisions. Nor do all reasoned decisions hinge upon basic considerations like the size of the hall in this example.

Two further points may be noticed about this sense. First, it will not accommodate the concepts of motive and character that are most often invoked as factors determining action, or at least it will not do so to the extent that determinism requires. For though a man's own character may, on occasion, be an important factor in his deliberations, as in choosing his occupation or his wife, this sort of consideration is rather seldom even relevant, let alone basic, to his decisions. Similarly, his own wishes may, but need not, be basic considerations in his reasoning, and may scarcely enter into it at all, as when he is primarily concerned to accede to the wishes of others. It seems clear, in general, that neither our own dispositions, *e.g.* generosity or laziness, nor our affective states, *e.g.* anger or fear, do determine the greater part of our conduct, in the sense of "determined" here discussed. We are not so introspective as we should have to be if this were the case.

Secondly, it would often make no sense to go behind the factors determining a line of action and to ask what *in this same sense*

determined them. For the factors may not themselves be the effect of a reasoned decision, and if so the "basic consideration" sense of "determined" has no application to them. Thus, the length of a bridge may be said to be determined by the width of a river, if the river's width is a basic consideration in deciding the bridge's length. But the width of the river is not itself the effect of a human decision, and thus cannot be said to have been determined by anything in the sense in which it determines the bridge's length. This is not, of course, to deny that the river's width could properly be ascribed to factors of one kind or another in an appropriate geographical context. It is only to deny that, in this particular sense of "determined", there need be an endless regress of determining factors behind every action that is performed. We need not contemplate a vista of determining factors of this kind stretching into the infinite past, and marching forward inexorably to any given state of the world. Yet it is, perhaps, vistas like this by which we are too easily hypnotised, when we are in the mood to be determinists.

A determinist might regard all the foregoing arguments as shadow-boxing. For, he might say, the important sense of "determined" is still different from the senses so far discussed. The only sense that matters for him is that in which some things are said to be determined by certain other things in accordance with laws of nature. The colour of a child's eyes, for example, is determined by the colour of its parents' eyes. Its sex is determined by the combination of chromosomes in the fertilised ovum. In such cases, laws have been discovered according to which some things are determined by others. Quite evidently certain kinds of human behaviour can be subsumed under such laws. For it can already be shown that various features of personality are determined by factors of heredity, physique and environment. It is not unreasonable, therefore, to regard all behaviour as amenable, in principle, to explanations of these types. One day, perhaps, we shall see that all conduct is completely determined, for the natural and social sciences will tell us exactly what determines it.

Now it does seem that here at last we have a sense of "determined" which, unlike the senses discussed earlier, has absolutely no connection with human power to control or decide anything. For characteristics like eye-colour or sex are determined *naturally*. The connections between them and the factors that determine them are independent of human will. This is exactly what the determinist needs. For in asserting that all actions are determined, he means to accord to them just this independence of the

will that seems to be implied in saying that eye-colour or sex are determined by certain biological factors. It is in this sense, then, we will now suppose, that the determinist holds that all actions are determined. We must therefore try to elucidate the meaning of "determined" in such contexts. For this purpose, it will be profitable to compare these assertions with others of a similar form where human decision and control are still clearly involved. Consider the three following examples.

- (1) The size of an employee's pension is determined by his salary on retirement and his length of service.
- (2) The date of Whit Sunday is determined by that of Easter.
- (3) The toss of a coin determines which team shall have the privilege of choosing whether to bat or field first.

These assertions appear to have three features in common. First, each implies that some rule exists for settling the answer to a question. Thus, in saying that a man's pension is determined by his retiring salary and length of service, we allude to, though we do not state, a rule for settling the question what his pension is to be. Some regulation lays down a formula for computing pensions from data about salary and length of service. Similarly, there are rules to the effect that Whit Sunday shall be the seventh Sunday after Easter Day, and that the captain who "wins the toss" shall choose whether to bat or field.

Secondly, if these different factors can properly be said to determine their respective issues, then we must, if we are to apply the rules to any particular case, first answer questions relating to these factors. Thus a man's retiring salary and length of service are said to determine his pension because the questions "What was his retiring salary?" and "How many years has he served?" have to be answered if we are to apply the rule and find out what his pension is to be. In saying that one thing is determined by another, or others, we are saying that the answer to one question requires an answer to another question or questions.

Thirdly, the question which the rule or convention provides a formula for answering is, in all these cases, a question about *what is to be done*. The question "What is a man's pension to be?" is a *practical* question, which pension regulations exist for answering with fairness and consistency. Similarly, rules exist for settling the *practical* questions "When is Whit Sunday to be observed?" and "Which side is to have the privilege of deciding whether to bat or to field?" The rules in all these cases are made by men for their own purposes, and are thus always liable to be

revised or abandoned by them. It will be convenient to say that issues settled in accordance with such rules are determined conventionally, by contrast with characteristics like eye-colour or sex that are determined naturally. In what follows, these two kinds of situation will also be referred to as cases of conventional and natural determinacy respectively.

It may now be noticed that in the conventional cases just discussed the purpose of the rules is to facilitate not prediction but decision. Pension regulations do not exist for predicting what a man's pension *will* be, but for deciding what it *shall* be. The rules of the Church's calendar or the conventions of cricket matches exist not for predicting the dates of festivals or the privileged teams, but for deciding when festivals shall be held, and which the privileged teams shall be.

Stress is here laid on decision, as opposed to prediction, because it will be argued shortly that modern discussions of this topic place undue emphasis on prediction. For the moment, however, let us notice one further point. In all the examples being considered, a knowledge of the rule would enable us to deduce the determining circumstance from the determined one just as well as it enables us to deduce the latter from the former. Thus, if we know both the convention linking Easter with Whit Sunday and the date of Whit Sunday in a particular year, we can work out the date of Easter in that year just as well as we can work out the date of Whit Sunday from that of Easter. Yet it is Easter's date that determines Whit Sunday's and not the other way around. This, presumably, is because the convention linking the two dates exists for settling the date of Whit Sunday but not for settling that of Easter. Similarly, it is because the regulation connecting a man's salary and length of service with his pension exists for deciding what the latter shall be that those factors are said to determine it. Here again it would be possible, given the formula and a knowledge of his pension and length of service, to work out the amount of his retiring salary. But his pension and length of service certainly do not determine his retiring salary. To make this point is, indeed, only to reiterate the distinction noticed earlier between saying that one thing is determinable *from* and saying that it is determined *by* another thing.

Our discussion of these examples can now be applied to natural determinacy. To begin with, it may be seen that one account of the matter sometimes offered in modern discussions is unsatisfactory. For example, Mr. Bernard Mayo, in his book *Ethics and the Moral Life* (p. 221), writes as follows :

The answer [*i.e.* to the question whether the claims of scientific determinism are valid for all actions] is, up to a point, already a commonplace. It consists in drawing attention to a distinction between the logical function of the term "determine" in scientific contexts and certain irrelevant associations of the word and its etymology. These associations are those which the word "determine" shares with words like "compel" and "force". When one event determines another, in the sense of causing it to happen, it is certainly not a case of compelling. *All that is meant is, that from the occurrence of the first event we can predict the occurrence of the second* (my italics).

Mayo does not enlarge on the commonplace distinction between "the logical function of the term in scientific contexts" and those "irrelevant associations" to which he refers. Presumably he means to identify the use in scientific contexts with that which he explains in the last two sentences quoted. But in any case, his explanation of that use seems inadequate. For if to say that one thing determines another means *only* that the latter can be predicted from the occurrence of the former, it would presumably follow that an event was determined by any state of affairs from which it could be predicted. But this is obviously not the case. Suppose, for example, that the Saturday attendance at a certain theatre is regularly double the week-day attendance. If we knew this, and if we knew the week-day attendance in a given week, we could predict the Saturday attendance for that week. But this would not mean that the Saturday attendance was determined by the week-day ones. It would be determined, if at all, by factors of quite a different type.

Moreover, we have seen that where one thing determines another in accordance with a rule, inferences may proceed from the thing determined to the determining factor just as well as in the opposite direction. This applies equally to natural and to conventional determinacy. Thus, if the laws of heredity are known, reliable inferences as to a parent's eye-colour can often be drawn from the colour of a child's eyes. It would follow, on the view being considered, that we might as well say that the colour of the parent's eyes was determined by that of his off-spring as the reverse. But this, surely, would be as paradoxical as saying that a man's retiring salary was determined by his pension. Determinacy is, in general, an asymmetrical relation.

This criticism might be countered in two ways. It might be objected, first, that eye-colour is not an event, and that since Mayo was discussing the determining relation between events only, this example should not be used to impugn the account he



gives of it. But clearly this will not help him. For it would not be difficult to find an example of two events such that while each could be inferred from the other, each could not be regarded as determining the other without generating as much paradox as it does in the eye-colour example. But, more important, the suggested objection brings out the impropriety of saying that an event determines another event "in the sense of causing it to happen". For "determined" is not ordinarily used in the sense of "caused" or "caused to happen". Thus, careless driving causes but does not determine traffic accidents; and building costs are determined but not caused by the level of wages. It is true that philosophers frequently use the words as if they were interchangeable. But since the associations of "determinism" are ever present, their use of "determined" only strengthens the feeling they are often trying to dispel, that some events really do seal the fate of others. In any case, the objection raised above to Mayo's account of "determined" would apply with equal force to any similar account of "caused", which is likewise asymmetrical. Finally, the eye-colour example shows that determinacy is not *essentially* a relation holding between events. Some account is therefore needed of the relation as holding not only between events but between circumstances of any other kind.

Our criticism might, however, be countered in another way. Stress might be laid on the word "predict", and it might be argued that whereas a knowledge of the parent's eye-colour enables us to predict that of the child, we cannot from our knowledge of the child's eye-colour predict that of the parent. It is, therefore, the predictability of one thing (B) from another (A) that entitles us to say that A determines B, and the asymmetry of the determining relation is thus preserved. But this too seems inadequate. For we have already seen, from the theatre-attendance example, that the predictability of B from A is not a sufficient ground for saying that A determines B. A further example will also show that it is not even a necessary condition for saying this, that predictability need not enter into the determining relation at all. Thus, the length of a shadow is determined by the height of the object that casts it and the elevation of the sun. But if we were looking at a shadow, it would be quite inept to say that a knowledge of these factors enabled us to *predict* its length. For one cannot predict a state of affairs that already obtains. All one can do in such cases is infer from available data a conclusion not previously known; and this is not in itself sufficient reason for saying that the state of affairs given in the data determines that which is reported in the conclusion. No doubt it is true that

where one thing determines another, the latter is determinable from the former. But, and the horse should be dead by now, the converse does not hold true. Therefore to say no more than Mayo does is to give an insufficient account of the determining relation.

We must therefore ask, once again, what is the meaning of "determined" in the contexts we are considering. For a positive answer let us now return to the three features of conventional determinacy noticed earlier. We saw, first, that in saying that one thing determines another we allude to, though we do not state, a rule which connects them. The same holds true for natural determinacy. In saying that a child's eye-colour is determined by the colour of its parent's eyes, we allude to, though we do not state, a set of "rules", the laws of heredity, connecting specific combinations of parental eye-colour with specific eye colours in their off-spring. In natural as in conventional determinacy a rule lies in the background of an assertion that one thing determines another.

It is clear, secondly, that we are saying in the natural, as in the conventional cases, that the answer to one question requires an answer to one or more other questions. In saying that eye-colour is determined by parental eye-colour we mean that the answer to a question as to the colour of a child's eyes requires an answer to questions as to the colour of its parents' eyes. The sense of "requires" here will have to be clarified shortly. For the present it need only be noted that natural and conventional determinacy have in common the assertion of a relationship between the answers to different questions.

But if we look at the third feature noticed in the conventional cases, there is apparently an enormous difference between them and the natural ones. For whereas the amount of a man's pension is a practical question, no such questions arise over the colour of a child's eyes. Man makes rules for settling his practical problems. But practical problems are, as far as we know, his privilege.

Nevertheless, language suggests that Nature has them too. In saying that the colour of a child's eyes is determined by that of his parents', we speak *as if* the question whether John's eyes are to be blue or brown were a practical issue for Nature to settle. Thus, to deal with the matter, she, or perhaps some clerical minion of hers, has only to ascertain the relevant data ("What colour were his parents' eyes?") and then apply the rule ("Off-spring of blue-eyed parents are to have blue eyes"). The determining factors correspond in this picture to the particulars of salary and length of service required by the clerk who computes a

pension. The question "What colour are John's eyes to be?" might be called a "quasi-practical" issue, by contrast with questions regarding his pension or nationality, that have to be settled by human decision.

It will be seen that the view of natural laws implied by this suggestion differs from that which treats them merely as techniques or licences enabling us to infer conclusions from relevant data, and thus to make predictions. No doubt they do do this. But this is not the picture of their function implied in our use of "determined". This use treats them not as licenses for our inferences, but as rules which Nature applies for settling questions about what is to happen. Hence we can now understand the sense in which the answer to a question as to the colour of John's eyes "requires" an answer to questions as to the colour of his parents' eyes. This sense is not that in which *we* might be said to "require" an answer to the latter question before we could answer the former. For to say this would ordinarily be absurd. There are plenty of ways in which we should usually be able to discover the colour of a person's eyes, without worrying about those of his parents. Rather the sense of "requires" that concerns us is that in which specific factual data are "required" for settling practical issues in accordance with rules laid down to cover them.

If this is correct, it helps to explain our usual tendency to suppose that determining factors must temporally precede, or at least cannot succeed, that which they determine; *propter hoc ergo non ante hoc*.<sup>1</sup> In this connection, mention may once again be made of a line of argument envisaged, though not actually adopted, by Mayo. He writes (*op. cit.*, p. 227):

And after all, if we are prepared to argue that the present is determined by the past, why should we not be prepared to accept that the present might be determined by the future? Indeed this can be argued even with respect to mechanical causation [i.e. not only on teleological grounds]. The distance of the moon from the earth a century hence can be inferred from their present state plus the laws of motion; but it could be just as easily inferred from the state of the bodies a thousand years hence. The laws of motion are neutral as to time; *it is only our ignorance of the future which generates the prejudice that mechanical determinism has a one-way time-dimension* (my italics).

<sup>1</sup> The following remarks are an excursus on to the battle field of Messrs Dummett, Flew and others in *P.A.S., Supp. Vol. XXVIII* (1954), and in *Analysis*, 1956-57. The explanation here suggested for the temporal priority of determining factors may, however, be too simple to deal with the same problem for "causes".

In the light of our earlier discussion, this does not seem right as it stands. For, we have argued, the fact that one thing is determinable from another does not warrant the assertion that it is determined by that other thing. If this is correct, our ignorance of the future cannot *alone* explain our reluctance to believe that it could determine the present. For even if we knew more of the future than we do, we should still not be justified in saying that future circumstances determined present ones merely because present ones could be inferred from them. Nor, indeed, should we want to say this, any more than we want to say *now* that historical facts were determined by the later circumstances from which we can infer them. Thus, the fact that the date of a man's death can be determined from an inscription neither entitles nor disposes us to regard the inscription as determining the occurrence of his death at that date.

The problem about the temporal order of determining and determined circumstances can be better dealt with, however, in the light of our comparison of natural with conventional determinacy. We have suggested that determining factors in nature correspond to the crucial particulars required for the application of a rule to settle a practical question. Now the data required for applying rules to particular cases generally relate to what has already happened, or is currently happening, rather than to what is going to happen. The data required for computing the pension of a man who is about to retire relate to his previous years of service and his current salary; not to such things as the number of years by which he will survive retirement, or the number of dependents he may have acquired at some date subsequent to it. Circumstances that will post-date the fixing of his pension are not relevant in computing it, for the regulations do not require us to take account of them. Similarly, therefore, in so far as Nature's laws do not require her to take account of the future in order to settle the present, we should not expect circumstances to be naturally determined by those that post-date them.

But, it may be asked, why should it always be the case that rules require us to take account of the past and present rather than of the future? Might it not be purely fortuitous that the factors determining the size of a pension precede the time at which it is computed. There is, indeed, an obvious reason for their doing so in this particular case—namely, that a pension is awarded to a man in recognition of services he has performed in the past, and not in anticipation of what he will do in the future. But this, it may be said, is a special feature of the example. In other cases it is quite conceivable that rules should require us, when we apply

them, to take into account circumstances that still lie in the future. If so, then nothing that has been said here really justifies our assumption that determining factors cannot succeed that which they determine.

It may be remarked here that the pension example is, in fact, by no means untypical. For pension regulations belong to the large class of rules that govern the reward, punishment, or equitable treatment of human beings. In all such rules, the past and present are crucially relevant in a way in which the future is not. There do, however, seem to be more fundamental objections to the idea that rules could ordinarily require us to take future circumstances into account when we are applying them. First, since we think of our present decisions as affecting certain subsequent events, it would be absurd to make those decisions turn upon the very events whose course may be affected by them. Since, for example, the number of years by which a man survives retirement may very well be affected by the size of his pension *inter alia*, it would be self-stultifying to insist that his pension be determined by the number of years for which he manages to survive.

Again, if we were to legislate that the length of an offender's prison sentence should be determined by his conduct record in the five years following conviction we should place the magistrate in a pretty quandary. For the length of an offender's sentence is itself likely to be one of the main things affecting his conduct over that period.

But there is a still more general difficulty. Clearly, if rules are to be of much value in helping us to settle practical questions, there must be a minimum of doubt and difficulty in applying them. A rule for deciding which team is to have the option of batting or fielding in a game of cricket could not reasonably require the answer to turn on, say, the state of the weather in the week following the game. A rule for settling such a matter must require us to take account of some simple circumstance, such as the result of a coin toss, that can be conveniently ascertained without delay and beyond doubt. Since we are rarely in a position to ascertain the future beyond all doubt, unless we wait to see what it brings, it is not surprising that rules do not ordinarily require us to answer questions about it, in order to settle practical issues in the present. To this extent, Mayo's suggestion that our ignorance of the future "generates the prejudice that mechanical determinism has a one-way time-dimension" may be accepted. Our ignorance of the future does bear on this "prejudice", in so far as it must *de facto* inhibit us from requiring practical decisions to turn upon future circumstances.

Let us now return to determinism, and try to reformulate it in terms of our analysis of "determined".

It can be seen that in two respects the assertion that all actions are determined does not fit very comfortably into the conceptual framework we have uncovered. For (a) we do not commonly speak of things just being determined *simpliciter*. We say, rather, that one thing has been determined *by* another thing. Thus building costs are determined by wage levels, and the trump suit in bridge is determined by the bidding. But it is not clear whether any meaning could or should be attached to these assertions if they were bereft of their last three words. (b) The noun phrases that stand as typical subjects of "... was determined" are not words standing simply for actions, such as "the posting of the letter" or "the murder of his aunt". They are phrases like "the time at which the letter was posted", or "the amount of arsenic he put in her tea", which are convertible into indirect questions *about* the actions ("At what time did he post the letter?", "How much arsenic did he put in her tea?"). It is more natural, therefore, to speak of the characteristics of an action than of the action itself as being determined. Yet it might well be doubted whether any enumeration of an action's characteristics could ever amount logically to a plain statement that the action itself was performed.

If we waive these objections, however, it would seem that to say that all actions were determined would be to say:

- (1) that all questions about all actions were settled in accordance with rules adopted by Nature for dealing with them; and
- (2) that for any such question there existed some set of circumstances crucially relevant for the application of the rule to it.

With regard to (1), the determinist would need a vastly greater number of natural uniformities than we now know of, to establish sufficient rules for his purpose. But there may be many more such uniformities than we now realise. There seems little point in debating whether enough of them exist to provide the background against which all our actions might be said to be determined, if they could be related to suitably "crucial" circumstances. We may also leave it an open question between what types of phenomena such uniformities might, if they existed, be found to hold. At present, owing to the success of a few types of explanation of character and conduct, we are inclined to disregard the possibility of any others. Thus we are very ready to sneer at the occult. Yet there might, for all we know, be a better case one day for saying that our characters were determined by our

stars, palm-lines or head-bumps, than there is *now* for saying that they are determined by our glands, toilet-training or sex-education. If uniformities could be discovered that would yield astrological or phrenological laws, and if those laws were constantly confirmed by subsequent experience, why should we not concede that our characters and fortunes were determined by stars or bumps to an extent undreamed of by those who now write horoscopes for the newspapers?

In making such concessions, however, whether to astrologists or to psychologists, there are two points to bear in mind. First, we need to remind ourselves of the extent to which analogy pervades the ordinary language on which determinism trades. If the questions which Nature adopts rules for answering are only, as we have suggested, quasi-practical questions, then the necessity governing her answers to those questions is only a quasi-necessity. In asserting that one thing determines another we are, it seems, projecting on to Nature the necessity that man-made rules impose upon man's actions. The concept of being determined is a boomerang, which is first thrown by language on to Nature from human conduct, and then returns to conduct again at the determinist's behest.

Secondly, it is worth remembering that man-made rules are made to be broken. There would be no point in having rules enjoining certain actions and prohibiting others, unless people were in fact capable of not acting as the rules require them to act, or of acting as the rules require them not to act. We should have no need of a rule prohibiting, say, drunkenness, if man were incapable of getting drunk. Nor do men cease to be capable of getting drunk merely because there is a rule forbidding them to do so. Obedience to rules is not guaranteed by their existence. They have to be enforced by penalties, or threatened penalties, and even such penalties and threats can only deter men from failing to comply with the rules. It is always open to individuals who are subject to a given system of rules, however strictly enforced, to decide in any particular situation whether they are going to abide by the rules or not. To say this is not, of course, to say that all rules actually *are* broken, but only that rules, even the best-kept ones, are essentially breakable.<sup>1</sup>

<sup>1</sup> Professor W. H. Dray has pointed out to me that not all man-made rules equally well support the line taken here. Thus it may be argued that there is a sense in which the rules of chess cannot be broken in the way that the rules of a society can. Perhaps the laws of Nature are more like these than pension regulations. Or perhaps they are *sui generis* and not comparable with man-made rules at all. I do not suggest that it would be profitable in all contexts to regard them in this way, but only that this is the picture of their function implied in our use of "determined".

These considerations may be applied to things which we regard as determined in accordance with laws of Nature. For here too, even if the laws are in fact unbroken, this is not to say that they are unbreakable. Nature's executives may suspend them, or her clerical staff may bungle their application. Such possibilities are, it would seem, not only compatible with but implicit in the view that all actual events are determined in accordance with the laws. Any one who insists, in the name of Science, that natural laws are not breakable, should consider whether the word "determined" has any proper application to events covered by unbreakable laws, or even, still more radically, whether the expression "unbreakable law" does not itself involve a contradiction. If it does, the conception of man's freedom (to keep or to break rules) will be implicit in the thesis that all his actions are determined. This thesis will thus be unstateable unless its antithesis is presupposed.

This whole discussion may be felt to be inadequate either as a refutation or as a mitigation of determinism. For this thesis, it may be said, has been too narrowly and arbitrarily interpreted. A determinist could easily argue his case without using the single concept here discussed. From the notion of "cause" alone he could derive just as damaging a conclusion as the particular thesis we have examined. Why, indeed, should he not use the word "determined" to mean "necessitated" in whatever sense his argument had established that all actions were necessitated?

This objection might, in theory, be met by more extensive analysis. Causality might itself be suspected of involving, at bottom, as harmless a projection as we have suggested that determinacy involves. Conceivably, all concepts figuring in determinist arguments could be clarified sufficiently to show that no threat to "free will" was involved in the ordinary use of any of them. The determinist would then be debarred from using the key words of his argument in their ordinary senses, and his conclusion would be largely robbed of its effect. For if all ordinary associations had to be withdrawn from his words, no really paradoxical thesis, and perhaps no genuine thesis at all, could properly be stated in those words.

In practice, however, this attractive prospect is not much of an answer to the objection we are considering. For no agreed analysis of all relevant concepts exists at present. Yet until it does, such claims as those we have envisaged can rest only upon credit; and there may be nothing but prejudice behind the fashionable faith that common-sense will be vindicated in the end.



In any case, even if a complete analysis of all relevant concepts existed, the piece-meal refutation of determinism that it afforded would probably carry little conviction with most people. For a belief in determinism is not the prerogative of philosophers and is not likely to be dispelled, any more than it is produced, by rigorous argument. Often enough it is the effect of a mood, a recurrent feeling, suggested by personal experience, and exploited by writers of fiction, drama, or history. Against such opposition analysis will make little headway. It may also fail even when it is faced with real argument. For it may be that the anxieties involved in determinism can always be restated so as to resist the therapy offered by any particular analysis. The chestnuts are very old, and they may be too numerous to be pulled out of the fire by the analyst's tweezers. He can only behead the Hydra in one place at a time; and if it is indeed a Hydra his task will never be completed.

The present discussion, therefore, makes no claim to "dissolve" the problem. It has attempted only to disarm one treacherous concept, which has a philosophical theory built into its "intellectual" use. Even when the word "determined" plays no part in the determinist's argument, it commonly figures in his conclusion and it christens his thesis. Once perhaps, in its innocent childhood, it was violated by philosophers. Nowadays, in sophisticated maturity, it is capable of seducing us all, philosophers and intellectual plain men alike.

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## IV.—INTERNAL AND EXTERNAL PROPERTIES

BY TIMOTHY SPRIGGE

ARE there internal properties of things? By a thing I mean anything of a sort such that it is true that something of that sort exists, leaving it open whether all things are what would ordinarily be called particulars. By an internal property of a thing, I mean a property of a thing such that the thing could not but have it.

The whole question has been discussed more often with regard to relational properties. What I say is intended throughout to apply as much to relational properties as to non-relational properties. My discussion of this topic represents, for the most part, my reactions to the ideas of Mr. John Watling, that keen opponent of internal properties.

The question thus framed is far from definite in meaning.

Let us consider two things which might be meant by the assertion that things have internal properties. Firstly, it might be meant that some propositions of the form 'Fa' are necessarily true. Secondly, it might be meant that some propositions of the form 'Fa' are entailed by corresponding propositions of the form 'a exists'. The second I shall dismiss on the grounds that it is nonsensical to talk of propositions of the form 'a exists'.

The first view is, in my opinion, false. But at the moment I am less concerned to establish its falsehood than to consider the appropriate way to state its falsehood.

The ordinary way of expressing the contradictory view is to say that all propositions of the form 'Fa' are synthetic, and therefore contingent, but certain associations of the words "synthetic" and "contingent"—whether they are of their connotations or not is doubtful—give to the view a paradoxical air which I shall briefly point out. 'Charlie Chaplin has at some stage in his life been a human being' is a proposition of the form in question. So on this view it is synthetic. Now the contradictories of synthetic propositions are generally supposed to be conceivable. Yet the contradictory of this proposition is not conceivable. This is a paradox of the view that there are no internal properties in the sense in question. This is the first of my two topics.

The other topic is this:—although it is impossible to distinguish the internal properties of a thing from its external properties, understanding "internal" as above, and "external" as non-internal, yet the attempt to do so stems from awareness of some

distinction which there really is to be made. For there is some sense of *could*, in which there are some properties of a thing which it could not be without, and other properties of it which it could be without. If we use the word "internal" for the first kind of property, and the word "external" for the second kind of property, then there are internal and external properties. Thus Charlie Chaplin simply could not have been without the property of at some time in his life being a human being, but he could have been without the property of having at least four wives. I do not think that in the sense which I thus allow to the internal-external distinction, all properties are either on one side or the other, but certainly some are more internal and others more external. I shall make some attempt to analyse this distinction, and this will be my second topic, but until expressly stated, I shall be using the terms "internal" and "external" in the sense first mentioned. I thus bring together my two main topics, because I may approach the matter somewhat obliquely in what follows.

We may divide propositions into two classes—those which are about things and those which are not. Let us call the former particular propositions and the latter universal propositions. This however is only a convenience—I do not imply that all entities are particulars.

A criterion, of a sort, for distinguishing between particular and universal propositions is that the former require in sentences which are to express them, at least one word standing for a thing, whereas the latter do not. The proposition expressed by the sentence "T. S. Eliot is a poet" is a particular proposition. Such a proposition does not, however, have to be expressed by a sentence containing what is ordinarily called a proper name. The same proposition might be expressed either by the symbols "T. S. Eliot is a poet" or the symbols "The author of *The Sacred Wood* is a poet".

In this paper I adopt the following notation. The occurrence of an expression between double inverted commas shows that the reference is to symbols or a sentence. The occurrence of an expression between single inverted commas shows that the reference is to a concept or a proposition.

Propositions about particulars require for their expression in a sentence use of a word referring to a particular. But this is not to say that all such sentences express particular propositions. A universal proposition may—it seems—be translated into a sentence including a word standing for a particular. For example, "All men are mortal" may be translated into "All men in

London and all men not in London are mortal". If this latter sentence expresses the same proposition as the former, then a universal proposition may be, but does not have to be, expressed in a sentence which includes a word referring to a particular. But a particular proposition, such for instance, as "All men in London are mortal" may not be expressed except by a sentence including a referring expression.

It might, however, be doubted whether "All men are mortal" and "All men in London and all men not in London are mortal" do express the same proposition. The ground of this doubt would be that to believe the former does not, while to believe the latter apparently does, require some sort of acquaintance with London. In that case, presumably, the proposition expressed by the latter sentence would be a particular proposition. But it seems that the two propositions would be logically equivalent, in which case a universal and a particular proposition could be logically equivalent, which is paradoxical. For the present, then, I take it that there is but one proposition, and that a universal one.

Universal propositions divide into the synthetic and the analytic. 'There are no men over the age of one hundred and twenty' is synthetic, and 'There are no men who are not mammals' is analytic. To believe a universal proposition of either kind is to be disposed to pass from believing particular propositions of one sort about any set of particulars to believing particular propositions of another sort about the same set of particulars, at least that is my pious hope.

Our first main question is whether propositions about particulars may likewise be divided into the synthetic and the analytic, for it is only if such a distinction is possible that the distinction between internal and external properties is possible. The internalist—as I shall call the believer in internal properties—must hold that either there are analytic propositions ascribing properties to particulars, or that there are propositions ascribing properties to particulars entailed by propositions ascribing existence to these particulars.

I shall dismiss the second alternative briefly, while admitting that my dismissal of it begs the question to some extent. For I shall boldly assert that an existential proposition cannot be a particular proposition. That it exists, is not a thing one can say about a particular. The premise of Moore's argument, that one can meaningfully say of a particular that it exists because one can clearly meaningfully say of a particular that it might not have existed, is false. One cannot say of a particular that it might not

have existed. One thinks one can say this, because it appears to be the contradictory of a proposition to the effect that the thing necessarily exists, which one is anxious to contradict. But once one sees that the words "this necessarily exists" express no intelligible proposition, the wish to contradict it by saying "this might not have existed" also disappears. If it is accepted then, that an existential proposition is never about a particular, one must admit that an existential proposition cannot entail a proposition about a particular. From the proposition that there is something of a certain sort, or that something is of a certain sort, one cannot deduce any proposition about any particular thing. From a negative existential proposition or universal proposition, however, one can deduce a proposition about a particular thing, but this is beside the point. Thus, if the internalist is maintaining that some proposition ascribing a predicate to a particular follows from an existential proposition, his case is lost, and there are seen to be no internal properties. The only remaining possible form of internalism is that which maintains that some propositions ascribing predicates to particulars are analytic.

If we could establish that there are no analytic propositions about particulars, then the internalist view would be refuted.

Most of my argument will be directed to showing that propositions such as 'The author of *The Sacred Wood* wrote or otherwise thought up *The Sacred Wood*' or 'This river bed has at some stage had water in it' are either not about particulars or are not analytic. But there is apparently one special class of propositions about particulars to which it is difficult to deny analyticity. I shall consider this class first, in order to get them out of the way.

Consider the proposition about a certain table 'this is not-round-and-square'. It does seem that here we have an analytic proposition about a particular. If we can call not-round-and-square a property of the table, then it seems that it is an internal property of it, it would be self-contradictory to deny it of the table. In which case it is established that the table has at least one internal property, which shows that there are internal properties.

Yet this is surely not the sort of thing which anyone who talks of internal properties has in mind. For properties like this belong to everything.

Let us deal with the point thus. Let us say that there can be no predicate, and therefore no property,  $F$  such that ' $(x) (Fx)$ ' is analytic. Then we may deny that there are any internal properties while leaving it open whether there may not be analytic propositions of the form 'This is such and such' where 'this'

refers to a particular. But in such propositions 'is such and such' is not a genuine predicate.

We may then divide particular propositions into predicative and pseudo-predicative ones. The mark of a pseudo-predicate is that necessarily it applies to everything, or, if a relation, that it relates everything. We may also call pseudo-predicates those which, necessarily, apply to nothing.

I take it that in denying that any predicative proposition is analytic, I am denying that there are internal properties.

But after all are not some of these propositions analytic? Is it not analytic that the author of *Waverley* wrote *Waverley*—using "write" in a way sufficiently sophisticated to rebut Moore's naivety?

Now 'The author of *Waverley* wrote *Waverley*' might express an analytic proposition. It might express the proposition that if anything is the only author of *Waverley*, then that thing is *an* author of *Waverley*. To believe this proposition is to be disposed to pass from any predicative proposition of the form '*x* is the author of *Waverley*' to a certain predicative proposition of the form '*x* is an author of *Waverley*'. That is, it is to be ready to classify anything one has classified as the only author of *Waverley* as an author of *Waverley*. It would be an analytic proposition because the success of the disposition in which believing it consists comes about in a manner different from that in which, for example, the success of a disposition to pass from any proposition of the form '*x* is called Thomas Stearns Eliot' to a certain proposition of the form '*x* wrote *The Sacred Wood*' comes about.

But "The author of *Waverley* wrote *Waverley*" might be used to say the same thing as is said by "Scott wrote *Waverley*" and this thing is clearly no analytic proposition. So it seems that if we interpret the sentence "The author of *Waverley* wrote *Waverley*" as about a certain particular (i.e. the particular who wrote *Waverley* and is known as Scott) then it is not analytic. That is, if it is used to give a description of a certain thing it is not analytic.

This point may be rammed home by saying that, after all, the author of *Waverley*, that is, Scott, might easily not have written *Waverley*, and therefore it is a contingent fact that—and therefore a synthetic proposition that—he did write *Waverley*.

But to ram home the point in this way is to raise the paradox upon which I remarked at the beginning. For it invites one to consider whether Scott could have been the very same person, and not have written *Waverley*, which, on consideration, one must grant he could have been. In this way one is invited to consider

any proposition about Scott which one might be tempted to think analytic, and to reject its claim to analyticity on the ground that one can imagine what it would be like for Scott not to have had the property predicated of him in the proposition; and one is forced in each case to think that indeed he could have been without these properties. Scott could have been born of different parents, have been famous as a composer rather than as an author, have lived in a different country. But could Scott have been a pig? When we answer that he could not have been a pig, that is, not throughout his whole life have been a pig—for the question whether he might have been at some stage transformed into one by magic like the sailors of Odysseus is another question—we are surely making a logical rather than a factual claim. Or again, consider a very full description of St. Paul and a very full description of Scott. Surely it is not even logically possible that the descriptions might have applied differently, that which in fact applies to Paul applying to Scott and *vice versa*.

This whole approach is wrong. For it presupposes a distinction between predicates which necessarily apply and those which apply only contingently, between internal and external properties. It attacks the idea that things have internal properties, not at root by a criticism of the concept of an internal property, but by showing that supposed internal properties do not necessarily qualify their subjects.

On the one hand there are those who say that it is always a synthetic proposition that a thing has certain properties. On the other hand there are those who say that for a thing to be at all, it must have certain properties. If there were not a thing exemplifying these properties that thing would not be at all. The former regard the latter as superstitious because they think that they cling to some notion of each object having an essence. The latter regard the former as superstitious because they think that they have some idea of a bare particular which is only contingently connected with certain properties, although it could as well have existed with any others instead.

The argument may proceed this way. The anti-essentialist challenges the internalist to find any properties which, say, the Queen must have had. The internalist suggests, perhaps, that the Queen must have the property of at some stage in her life being Sovereign. The other suggests that it is only a contingent fact that the Queen did not die at birth, and therefore only a contingent fact that, and so a synthetic proposition that, the Queen has the property at some stage in her life being Sovereign. The internalist then says that the Queen must have been born of

Royal Blood. The anti-essentialist says that there would be no contradiction in a news bulletin asserting that it had been established that the Queen was not in fact the child of her supposed parents, but had been secretly adopted by them, and therefore the proposition that she is of Royal Blood is synthetic

In this way the anti-internalist parries the argument of the internalist by suggesting with regard to each proposed internal property of the particular in question, that we can quite well imagine that very same particular without the property in question.

For a time he is winning. Yet there comes a time when his claims appear a trifle too far fetched. The internalist suggests that we cannot imagine that particular we call the Queen having the property of at no stage in her existence being human. If the anti-internalist admits this, admits that it is logically inconceivable that the Queen should have had the property of, say, always being a swan, then he admits that she has at least one internal property. If on the other hand he says that it is only a contingent fact that the Queen has ever been human, he says what it is hard to accept. Can we really consider it as conceivable that she should never have been human?

Here then the internalist is winning. But it is not an altogether happy victory, for the properties which he may win for the Queen as internal to her are of a somewhat unspecific nature. The properties which can be won for her as internal are hardly such as distinguish her from her courtiers.

The internalist can make a rather better case for a somewhat different sort of particular. But it should not be thought that the difficulties he had in ascribing individuating internal properties to the Queen arise from special problems connected with the criteria of personal identity. Usually it is as difficult to find specific internal properties for individual physical objects.

Consider now a particular experience. At a certain specific time a certain person A has a certain particular experience E. It is an unpleasant gustatory sensation caused by having got some soap into the mouth, in the course of shampooing his hair. A can to some extent describe his experience, and in so doing he is clearly describing a particular. It was a nasty bitter taste, or perhaps rather the consciousness of a nasty bitter taste. Now the anti-internalist would say that that very same particular might logically have been a sweet taste, rather than a bitter taste. But the internalist would surely speak reasonably if he said that to suppose this, is to suppose that the particular in question might never have occurred at all, but some other



particular have occurred instead. *That* particular taste could not conceivably have been a sweet taste—its bitterness was a property internal to it.

In order to deny internal properties the anti-internalist finds himself forced into quite unpalatable contentions, such as that that very same taste might have been sweet and not bitter. Also he has to make even odder claims such as that it is only a contingent fact, and therefore a synthetic proposition that, Napoleon was a human being and not a boat. All this he seems forced into, when he wishes to deny that any particular thing necessarily has some of the properties which it has.

He is making a valid logical point, but somehow his whole approach is wrong. For it tacitly accepts a distinction between predicates which necessarily apply and those which apply only contingently, internal and external properties, and attacks the idea that things have internal properties, not at root by a criticism of the concept of an internal property, but rather by attempted impeachment of any proposed internal property, as lacking the credentials, that is as not being a property which the particular necessarily has.

If by an internal property of a particular is meant a property such that the proposition that the particular exemplifies it is analytic, then there are no internal properties. But this has nothing to do with any indefiniteness in our concept of the particular in question, but rests upon the nature of predication.

If one considers the sentences which express propositions the point becomes almost grammatical. In sentences expressing particular propositions where the subject word is a name, the subject word has no connotation. Therefore no predicate word can have a connotation which is incompatible with the connotation of the subject word. But a subject-predicate sentence could only express a necessary proposition if the connotation of the subject word were incompatible with the connotation of the negation of the predicate word. Since this cannot be the case where the sentence expresses a particular proposition, no sentence can express a proposition ascribing properties to a particular which proposition is necessary. From this one may conclude that there are no such propositions and hence that particulars do not have internal properties. Of course, this rests upon the questionable view that there may be naming words without connotation—and this indeed is basically the point at issue. Our discussion may at least remove certain reasons for refusing to accept that there may be such names, for it may show that it does not have the paradoxical consequence that anything might have had any

properties whatever, at least that it does not have this consequence in a form which is paradoxical.

To express a proposition about a particular is to describe or evaluate something. We will consider only descriptive propositions for the present. Such propositions merely classify the thing as of one sort or another. It is quite inappropriate to seek for some properties which the thing must have to be what it is. But one does not quite like admitting this, because it seems to draw a division between substance and properties, and to suggest that the connection between them is somehow fortuitous, and that the substance could just as well have quite different properties.

But it seems that one must identify a thing by some description, and having been thus identified as answering to that description, is it not in effect defined as the thing having those properties, which properties therefore it necessarily has?

One may remove these doubts, I hope, by considering a conversation such as follows. It is not a very ordinary conversation, but it is quite intelligible. A: I am going to describe a certain thing to you. B: What thing? A: You will recognise it as the description proceeds. It is domed. It is made mainly of stone. It is partly hollow. It is in the heart of a great city. It houses many famous paintings. B: You're talking of the National Gallery. A: I will continue. Entrance to it is free. In front of it is a square containing a tall column on which stands the statue of a famous sailor who is looking out upon the capital city of his country.

Now in enunciating these properties A was describing a certain object. A had to give a certain amount of the description before B cottoned on to what he was saying. But the properties of the National Gallery, the mention of which in A's description enabled B to identify it, are not properties of the National Gallery in any logically different way from those mentioned after B had managed to identify the object. If one sets out to describe an object one cannot distinguish between properties which are essential to it and those which are not. It simply is a thing of a certain sort, and that is that.

But people find it difficult to see how the object can so to speak get into the proposition except as an object, or the object, answering to a certain description. A serious attempt to deal with this point would require an analysis of the concept of a proposition. But some light on the nature of a predicative proposition may be thrown by consideration of a simple situation in which one is formulated.

Someone notes a passing object and says 'That is a starling'.

Here the speaker nearly uses the object as its own name. If he caught the bird he might almost hold it up and say '—— is a starling'. When one says that a passing object is a starling, one's belief that it is a starling is a way of treating a certain actual object. One is classifying it, the very object itself, that is, roughly, one is behaving towards it in a manner such as one does towards certain other, but not all other, things which one comes across. To hold a belief of this sort is quite different from holding a universal belief, for in this case one's belief is a way of treating an actual thing, and one can only believe a proposition about that thing by treating it itself in a certain way. But a universal belief is a second order disposition, a disposition to pass from one's treatings of things in one way to treatings of them in another way, and this second order disposition can exist—in the manner of dispositions—without ever being activated by any treating of an actual object. Thus actual objects figure in particular propositions, which are the internal accusatives of beliefs, as they do not in universal propositions.

This account does all very well for the belief in particular propositions where the particular in question is sensibly present to the believer, and may itself be classified in some way or other. Then we may perhaps consider a belief about the particular as being a certain treating of that particular. But what about belief in propositions about particulars which are remote in space and time?

Bertrand Russell at times held that we could only believe propositions about particulars which were sensibly present, and we would get out of our difficulty if we followed him here. But this would be to give up—as he did—treating "Julius Caesar" as a word used by us to name a particular, and I do not think we would be right to do this.

Rather, I would say that in believing propositions about Julius Caesar we are still reacting to him, to the man himself, in some way, and treating him, when for instance we believe him to have been a dictator, in a way such as we treat some other but not all other objects. It is something of the same kind as our classification of a passing object as a starling, and as such basically different from believing a universal proposition.

However this may be, to describe an object cannot be to formulate an analytic proposition. One may gather from someone's description what object it is of which he is talking. But there is only one way of describing an object—that is saying what sort of thing it is, and one cannot distinguish some such sayings as analytic.

Yet to say that a proposition descriptive of a particular is *synthetic* is liable to mislead. To say that it is synthetic suggests the thought that logically it might have been otherwise. For instance consider the proposition 'it is a building'—where 'it' refers to the National Gallery.

If we say that this proposition is synthetic it suggests that it is logically possible, that is conceivable, that the National Gallery should not have been a building. Then we worry ourselves as to what it would have been like for the National Gallery not to have been a building. For if it is a synthetic proposition that it is a building, it must be logically possible that it should not have been a building, and if it is a synthetic, though false proposition, that it is not a building, we should be able to imagine what it would have been like for it to be true.

Here we have the source, I think, of some of the worry that the concept of a substance produces. For if it is a synthetic proposition with regard to any substance, or particular, that it has any property, it seems that it could have been the very same substance while having quite different properties. This makes transubstantiation possible—though this is actually the reverse, a change of substance without change of properties—but leaves the substance as an unknown something.

If every proposition has to be synthetic or analytic then it is better to call the description of a particular, that is a predicative proposition, synthetic. But to call predicative propositions synthetic is misleading, in that it suggests the thought that one could imagine what it would be like for them to be false.

Here is a certain thing, let me try to classify it. That is the spirit in which a description is properly made. It just is a thing of a certain sort which sort the description—the assertion of the predicative proposition—attempts to specify. To ask whether a world is conceivable in which this very thing was quite otherwise is bewildering.

Certain descriptions apply to the thing. Once one has noticed that they apply, one does not always know what to make of the question—must they have applied?

Thus it would be in some ways more suitable to say that the analytic-synthetic distinction does not apply to predicative propositions than to say that they are synthetic. They cannot properly be called analytic. On the other hand one cannot—in many cases—properly ask what it would be like for them to be false. If, therefore, we do call them synthetic, it should only be as a way of contradicting the claim that any of them are analytic.

But despite the fact that descriptions of a thing cannot be divided into expressions of analytic, and expressions of synthetic propositions, yet there is a sense in which propositions of the form 'Fa' may either ascribe to a thing a property such as it could have been without or may ascribe to a thing a property such as it could not have been without. For instance, this chair has both the property of being a chair, and the property of being in this room between 3 and 4 this afternoon, but whereas one would want to say that it could easily not have been in this room at the time in question, it could not have failed to be a chair. Similarly Macmillan could easily not have been Prime Minister in 1959, but it is not easy to see that he could have failed to be a man, and perhaps not easy to see that he could have failed to be British. What account can we make of this sort of distinction—a distinction at which those who distinguish between internal and external properties have perhaps been aiming? For what they have been suggesting is that while it is rubbish to suggest that Macmillan might not have been human, it is not rubbish to suggest he might not have been Prime Minister.

Someone might ask whether the Nelson Column could be painted gold and retain its brightness. This is a question the answer to which would depend upon how London's atmosphere affects gold paint. That is to say, among the predicates which may apply to a thing are many such as 'would soon look dirty if painted gold', 'would burst if pricked', 'would hit you if you said that', 'would die if injected with such and such'. Many questions of the form 'could this thing be different from what it is?' may be answered by drawing attention to the predicates of this sort which apply to it. The question—'could Scott have written music instead of novels?' would be answered if we could decide that certain predicates applied or did not apply to him, e.g. 'would have written beautiful music if he had been given a training in harmony', 'would not have written his novels if he had had more money'. In short, there are all sorts of truths about particulars of the general character 'x would under such and such circumstances not have had such and such properties which x now has', or 'there are no circumstances under which x would have had such and such a property'. So we may make a distinction between properties which a thing would have under any circumstances and those which it would not have had granted certain other circumstances.

But to ask whether a thing could have been quite different from what it is, whether Scott could actually have had all the properties of Handel, is on a different level. The questions we

have just been asking are all to some degree requests for further descriptions of Scott. But the present question is not one that calls for any investigation of Scott, and it is difficult to accept that a question which calls for no investigation of Scott, to which nothing about Scott is relevant, is really about Scott.

What I want to understand is why, while we can accept the idea that Scott might have written music rather than novels, we cannot accept the idea that he might have had all Handel's properties, and Handel have had all Scott's.

But our question does not have to be asked about persons. Some philosophers have thought that this sort of problem was especially perplexing about persons, but the perplexity with which I am at present concerned should be provoked equally by meditations upon pots and pans, ideas and feelings.

Describing a particular is classifying it, saying that it is of one sort rather than another. Now in some cases to say of a particular that it might have been different from what it is, saying that it might have had a property F, which is incompatible with G, a property which it has, is one way of classifying it. That is, to say it might not have had property G, or more specifically, would under specified conditions have had property F, is in some cases to tell one something true about how the object actually is.

Suppose someone says of a certain lawyer, on the basis of hearing him sing at home, that he might have been a great opera singer. This is to classify him in a certain way just as to say that he is a very bad lawyer is to classify him in another. Suppose one says of him that he might never have been a lawyer. This is somewhat vague, and one would probably require elucidation of it if it were ever said. A very general interpretation of it would be that at an earlier stage in his life there was much to suggest that he would not become a lawyer but something quite different. It might, however, be a prelude to some very detailed story concerning how his becoming a lawyer was a result of some quite unexpected circumstances.

To say that Scott might have composed music rather than written novels, might be to make a variety of claims. It might be to say that he belonged to a class of men who may become either very good composers or very good novelists, and that which they become depends upon external circumstances.

My general point is this: to say that something might have had a property F, which is incompatible with a property G which it does have, may be a way of saying something quite true about the sort of thing it is. But there are many cases when to say

such a thing is not to say anything true about the sort of object it is. Sometimes indeed philosophers may say that a thing might have had such a property *F* as a way of saying that the proposition that it has property not-*F* is not analytic. But this—as we have seen—is a misleading way of making the point.

But we wished to distinguish between internal and external properties. I suggest that a property is internal to a particular to the extent that no information about that particular is conveyed by one who says that it might have lacked that property. I think that the distinction between internal and external properties is not exact. Now it seems that to say that Napoleon might not have been human, and perhaps that he might not have been Corsican, is to convey no information about him. But to say that he might have won the battle of Waterloo might well be said in a context in which it conveys information. To say that Caesar might not have crossed the Rubicon might really convey information about the sort of man he was, it might tell us that there were many motives in him working against his crossing the Rubicon. But to say that he might have been Japanese does not seem to tell us anything about him. But perhaps it might be to say that many of his qualities were compatible with being Japanese. Anyway one has in each individual case to find out what is being said. The central point is that suggestions that a thing might have been otherwise are intelligible in so far, and only in so far, as they are attempts to classify the object as it is, or to say what would happen to it under specifiable circumstances.

I propose the following definition. Let *F* be any property of a thing *a*. Then *F* is an external property of *a* if something interesting and true may be said of the form 'if such and such then not-*Fa*'. Otherwise *F* is an internal property. But as from different points of view different things are interesting, so from different points of view different properties are internal and external.

The question has so far only been considered with reference to those things which are commonly called particulars. Are there any other things, for instance universals, and supposing there are, does what has been said apply to them also?

Suppose there is such a thing as the colour Blue. One may raise the question whether it can be denied that the proposition 'Blue is a colour' is both about a thing, Blue, and logically necessary. In this case there would be internal properties in the first sense, the sense in which I have denied that there are, though indeed only internal properties of non-particulars.

To avoid this, it might be argued that 'Blue is a colour' is a universal proposition to the effect that  $(x) (x \text{ has the property of being the universal Blue} \supset x \text{ has the property of being a colour})$ . This is analytic, but it is not about Blue. But if there is something which is the colour Blue, why cannot one talk directly about it? If one is not going to do so, one may as well forthwith equate the above proposition with ' $(x) (x \text{ is blue} \supset x \text{ is coloured})$ '.

Consider now a conversation like that I imagined earlier.

A. I am going to describe a certain thing to you.

B. What thing?

A. You will recognise it as I proceed. It is eternal. It is non-spatial. It is very beautiful. It is a colour. It is more like black than red. It is often exemplified by the sky.

At some stage B will recognise what A is describing, namely Blue. Or perhaps if he is a nominalist he will say that he doesn't think A has succeeded in describing anything. But presuming for the moment that A is describing something, his propositions are not analytic, even if he ends up by saying 'It is the property Blue'. The interest of this example is that it brings out forcefully the *manner* in which propositions about particulars are synthetic. It shows that the syntheticity of predicative propositions is nothing to do with the nature of the entities of which they are descriptive, nor anything to do with the separability in thought of a thing from its properties. Here it is a universal, rather than a particular, that is in question, and if propositions about universals may be regarded as synthetic, how much more may propositions about particulars be! The point is that although propositions about particulars such as the National Gallery or universals such as Blue are not analytic, and therefore perhaps to be called synthetic, it does not follow from their being synthetic that we may reasonably ask what it would be like for them to be false. It does not mean that we can imagine the National Gallery not being a gallery, or Blue not being a colour. They are what they are—and the question Could they be different? can only be answered by considering the applicability to them of certain other predicates of the type '— would have been F, if p'.

Yet it seems that if one does accept that 'Blue is a colour' is of the form ' $Fa$ ', and is not the same as either of the universal propositions above, one is landed with a thing having internal properties in the objectionable sense. For surely it is logically necessary that Blue is a colour?

I would dispute this. If in the sentence "Blue is a colour", "Blue" really is used to refer to something, namely the colour



Blue, then the proposition expressed is not analytic. The fact that it would not be analytic throws some light on the question of internal properties in the objectionable sense. For if anything had internal properties in that sense one might think it would be Blue. And this shows that the denial that a thing has internal properties need not be linked with any claim that in the case of each of its properties it could be conceived or imagined without it.

The reason for saying that 'Blue is a colour' is, if it is actually about Blue, not analytic, is that if it is actually about Blue it is a description, and a description of something cannot be the expression of an analytic proposition.

## V.—PRESCRIBING AND EVALUATING

BY PAUL W. TAYLOR

THE distinction between the role of the moral agent and the role of the moral judge is a familiar one. But how are the two related? In particular, what is the logical connection between telling someone what he ought to do and appraising, judging, or evaluating someone's conduct? In this paper I shall try to make clear the difference between these two sorts of activities and show how they are related to each other. For convenience I shall designate the first activity "prescribing" and the second "evaluating", although I do not claim that the concepts I am trying to elucidate exhaust all the ordinary meanings of these two terms. One conclusion of my study will be that an act of prescribing is justified by appeal to a set of evaluations. Each of these evaluations must in turn be justified if the act of prescribing is to be fully justified, but I shall not consider in this paper the justification of evaluations.

### I

I begin with an analysis of what we assert when we say that someone ought to do, or ought to have done, a certain act. This will serve to bring out the distinction between prescribing and evaluating, since I intend to show that an "ought" sentence may function in either of these ways. To claim this is already to deny that the difference between prescribing and evaluating is correlative with the difference between the ordinary use of "ought" and the ordinary use of "good" (or "right"). That "ought" sentences may express evaluations as well as prescriptions becomes clear when we take into consideration the person and tense of such sentences. Accordingly, I classify "ought" sentences as follows :

#### I. Particular sentences

##### A. *Ante eventum* sentences<sup>1</sup>

1. First person ("I, we ought to do X.")
2. Second person ("You ought to do X.")
3. Third person ("He, she, they, or those named or described in some specific way, ought to do X.")

<sup>1</sup> The terms "*ante eventum*" and "*post eventum*" are R. M. Hare's, *The Language of Morals*, p. 157.

B. *Post eventum* sentences

1. First person ("I, we ought to have done X.")
2. Second person ("You ought to have done X.")
3. Third person ("He, she, they, or those named or described in some specific way, ought to have done X.")

## II. Universal sentences

- A. Active ("One ought to do X in circumstances C.")
- B. Passive ("X ought to be done in circumstances C")

Whether an "ought" sentence is particular or universal depends upon whether the agent (*i.e.* the person designated in the sentence as the one who ought to do the act) is specified or unspecified. In a universal sentence the circumstances in which the act ought to be done are specified, but the person or persons who ought to do the act are not. The agent is *anyone* in those circumstances who can do the act. In a particular sentence, on the other hand, the agent is referred to by a proper name, by a personal pronoun, or by a definite description. In each case something more about the agent is specified or understood than merely that he is an agent.

A particular "ought" sentence is *ante eventum* when the act of uttering the sentence occurs before the act designated in the sentence. When the sentence is uttered after the designated act, it is *post eventum*. We shall see that this distinction is important for understanding how evaluations differ from prescriptions. For I shall contend that the uttering of a *post eventum* "ought" sentence is never an act of prescribing but is instead the expression of an evaluation. *Ante eventum* sentences, however, may be either prescriptive or evaluative, depending on the circumstances in which they are uttered and on whether they are in the first, second, or third person.

Let us first consider *post eventum* sentences. These sentences make sense only under two conditions: first, the person designated by the subject of the sentence (*i.e.* the agent) has been confronted with the choice of doing the act specified in the sentence or doing some alternative act, and second, that situation of choice has ceased to exist by the time the sentence is uttered. (The sentence is uttered *post eventum*.) Even in the case of a second person sentence, where the agent is the same person as the one to whom the sentence is addressed (hereafter, the addressee), the addressee is no longer in the position of an agent with respect to the particular act in question. To say "You ought to have done X" is to refer to a past situation in which the addressee had a

choice of doing X or not doing X. The statement contextually implies (1) that the addressee chose not to do X, (2) that the addressee was wrong in so choosing, and (3) that the situation of choice is now past. If act X were still open to the addressee's choice at the time of uttering the sentence, the sentence would be in the present tense and in uttering it (under certain conditions to be specified later) the speaker would be *prescribing the doing of act X* to the addressee. He would be telling the addressee what he ought to do and so would be guiding his present choice. He would not be *condemning (negatively evaluating) the addressee* for something he had not done in the past.

It is interesting to note that both positive and negative *post eventum* sentences express negative evaluations. Whether we say "You ought to have done X" or "You ought not to have done X", in either case we are condemning the addressee for his past choice. In the first sentence we blame him for not having done X, in the second for having done X. The first, in other words, contextually implies a "wrong of omission", the second a "wrong of commission". Can a *post eventum* sentence ever express a positive evaluation? Such sentences are almost never used for this purpose. In order to do this one would have to make the awkward statement, "You ought to have acted just as you did". It is much more natural to use an evaluative sentence with the predicate "right" to make the point, thus: "It was right for you to have acted as you did", or more simply: "You did the right thing". These sentences are ways of praising a person. They express a positive evaluation of the agent. They are not ways of prescribing an act for him to do.

Similar considerations hold for first person and third person *post eventum* sentences. In every case an evaluation of an agent is being expressed; an act is not being prescribed to an agent. When I say "I ought to have done X" I am condemning myself for not having chosen to do X in a past situation of choice. This negative evaluation of myself is based on an evaluation of my past act. The act is evaluated as one member of a class of comparison composed of the alternative acts open to my choice. By saying "I ought to have done X" I am acknowledging the fact that I chose not to do the best act open to me, which was act X, and *for that reason* I am blaming myself. I also blame myself when I utter the negative sentence, "I ought not to have done X". But this time I blame myself for what I did, not for what I did not do. It is an acknowledgment of a "wrong of commission" rather than of a "wrong of omission". The sentence contextually implies, not that an act *not* done *was* the best thing

for me to do, but that the act *done* was *not* the best thing for me to do.

Third person *post eventum* sentences also express negative evaluations of the agent designated in the sentence. They do not prescribe an act to the agent. In order to prescribe an act to the agent, the agent must be the addressee, since prescribing is *telling* the agent what he should do. But it is the mark of third person "ought" sentences that the agent is never the addressee. We may *indirectly* guide our own or others' choices by uttering a sentence of the type, "He, she, they ought (or ought not) to have done X". We would then be giving examples of acts which ought (or ought not) to have been done. Prescribing an act to someone, however, is not merely giving him examples of what he ought to do in various sorts of circumstances. It is to tell him what he ought to do when he has (or will have) the choice of doing or not doing what he is told. It is to provide a direct and unequivocal answer to the question, "What should I do?" Such an answer cannot be provided by a sentence of the form, "He, she, they ought to have done X", since it only tells a person how *others* should have acted in the *past*, not how the person himself should act in the present or future.

What about *ante eventum* "ought" sentences? Are they not all prescriptive? If we begin with third person sentences, we see at once that they cannot be prescriptive for the same reason that third person *post eventum* sentences cannot be prescriptive: they are never addressed to the agent designated in the sentence and so can never provide a direct answer to the question, "What should I do?" I suggest that third person *ante eventum* sentences are evaluative, as are all *post eventum* sentences. But they differ from *post eventum* sentences on two counts: (1) They express evaluations of the act, not the agent. (2) Positive sentences express positive evaluations and negative sentences express negative evaluations (instead of both expressing negative evaluations, as is the case with *post eventum* sentences).

When we say of a person, "He ought to do X", we mean that X is the best thing for the person to do in the situation of choice which now confronts him or in a future situation of choice which will confront him<sup>1</sup>. The claim that act X is the best thing for

<sup>1</sup> Kurt Baier has argued that the question "What shall I do?" means the same as "What is the best thing to do?" (*The Moral Point of View*, chap. 3.) It would follow that the answer to the one question ("You ought to do X") means the same as the answer to the other ("X is the best thing to do"). I shall point out below that these two statements are not equivalent, although I do think that "He (she, they) ought to do X" is equivalent to "X is the best thing for him (her, them) to do".

him to do is a judgment of X based on a process of evaluation in which X is shown, according to certain standards, to have more merits (good-making characteristics) and fewer demerits (bad-making characteristics) than any other act open to the agent in his situation of choice. That is to say, act X is *graded* or *ranked* as superior to all alternative acts confronting the agent, and as such it is judged to be the act which the agent ought to do. Unlike the sentence "He ought to have done X", which expresses a condemnation of the agent on the ground that he did not do X in the past, the sentence "He ought to do X" neither praises the agent for doing X nor condemns him for not doing X. How could it, since at the time of uttering the sentence the agent has neither done nor omitted doing the act but is still confronted with the choice of doing or not doing it? In saying of someone that he ought to do a certain act, we are not evaluating *him* at all. We are instead evaluating the act (as the best thing for him to do).

But the act, in being evaluated, is not being prescribed. The reason for this is that, although the sentence tells what the agent ought to do, it does not tell it *to him*. The agent is not the addressee. From the standpoint of the addressee, *he* is not being told what he ought to do; he is only being told what someone else ought to do. Or we can look at this from the standpoint of the speaker. In saying "He ought to do X" the speaker is stating what he thinks a certain person ought to do. He is not *telling* that person what he ought to do. But prescribing is at least doing this: telling a person what he ought to do. Therefore the uttering of a third person *ante eventum* "ought" sentence is not an act of prescribing.

Just as the positive sentence "He ought to do X" expresses the positive value judgment that X is the best thing for the agent to do in the presupposed situation of choice, so the negative sentence "He ought not to do X" expresses the value judgment that X is not the best of the alternatives open to the agent. Act X may be a right act (as being permitted or required by a rule of conduct) or a good act (as judged by its effects) but it is not, in comparison with the alternatives, the very best (or *the* right) thing to do. Hence it is judged negatively, as being less than the best. Similarly, when we utter the positive sentence "He ought to do X", act X may be a wrong act or a bad act, but when compared with the alternatives it is the least bad. That is to say, it is the best of the alternatives and consequently is taken to be the act that ought to be done.

In discussing first person *ante eventum* "ought" sentences I shall consider singular and plural sentences separately, for there

are some pertinent differences between them. First person singular *ante eventum* sentences are evaluative, not prescriptive. They are ordinarily used in the context of the speaker's deliberation. Professor P. H. Nowell-Smith has pointed out that, unlike "You ought" and "He ought", "I ought" contextually implies that the speaker has decided, or is trying to decide, what he ought to do. (*Ethics*, Penguin edition, pp. 261-262.) The sentence "I ought to do X" either sums up a deliberative process and informs the addressee that the speaker has come to a final decision, or it is uttered before deliberation has terminated. In the former case the speaker has carried out an evaluation of the alternatives confronting him (or has accepted someone else's evaluation of them) and has made up his mind concerning which alternative is the best. "I ought to do X" expresses a value judgment which is the outcome of the deliberative process. In the latter case, in which the speaker has not completed his deliberation (or has not accepted another's deliberation), he may say "I ought to do X" as a tentative decision, not a final decision. As Nowell-Smith puts it :

A man may hesitate between two moral principles and say to himself at one time 'I ought to do X' and at another 'But on the other hand I ought to do Y' or he may contrast 'I ought' with 'I should like to' (*Ibid.* p. 261.)

In both of these instances the "ought" sentence is evaluative, not prescriptive. For we deliberate (or accept someone else's deliberation) about what we ought to do when we are or will be in a situation of choice and are trying to decide what is the best course of action open to us. When we come to a decision, however momentary or tentative, we arrive at a judgment about what is the best thing for us to do. To arrive at such a judgment is the very purpose for which we deliberate. Indeed, the process of deliberation is nothing but the process of carrying out a series of evaluations concerning the various acts open to the agent in order to determine the best act.

When a first person singular *ante eventum* sentence is spoken to oneself (in which case the speaker, the addressee, and the agent are all one and the same person) it is tempting to say that the speaker is prescribing an act to himself. We are tempted to say this because, on the face of it, the speaker is telling himself what he ought to do, and to tell someone what he ought to do is to prescribe. Indeed, the parallel with ordinary prescribing is so strong that there is a point in saying that in this situation "I ought" really functions as an internalized "you ought" (the

phrase we normally use for prescribing). Nowell-Smith makes this suggestion in his analysis of cases where a person hesitates between two moral principles ("I ought to do X but on the other hand I ought to do Y") or between acting on a principle and acting on some other motive ("I ought to do X but I should like to do Y").

In the first case it is quite natural to represent the two 'oughts' as being spoken by internal moral authorities advising or telling him what to do; and in the second to represent the conflict as one between the Voice of Conscience and Desire. But these are the voices of advocates, not of judges; and what they say is, not 'I ought', but 'you ought'. (*Ibid.* pp. 261-262.)

Being an advocate and being a judge are the roles we take, respectively, when we prescribe and when we evaluate. And it is not incorrect to think of ourselves as advocating (prescribing) that we do one thing rather than another when we tentatively reach decisions during a deliberative process. But a caution must be interposed. Although reaching decisions during deliberation is like prescribing in some respects, in other respects it is not.

First, we are using the word "prescribe" out of its normal context. The same is true of Nowell-Smith's use of "you ought" in the above passage. Granted that prescribing consists in telling someone what he ought to do, it typically consists in doing this in a social context in which one person is offering guidance, making recommendations, or giving advice to another. Second, we must realize that in uttering the "ought" sentence to himself the speaker is not merely prescribing to himself. He is also pronouncing a judgment that is the outcome of his (or another's) evaluation of various acts open to him. It is only on the basis of the judgment "X is the best thing for me to do" that he then concludes: "I ought to do X." Third, if the latter sentence is taken to be an act of prescribing to oneself, the four necessary conditions for prescribing, which I shall consider shortly, must be fulfilled. When we utter such a sentence seriously to ourselves these four conditions in fact will usually be fulfilled.

A first person plural *ante eventum* sentence ("We ought to do X") may or may not be uttered in the context of deliberation. When it is, it functions in the same way as a first person singular sentence. The only difference is that in the case of plural sentences the speaker is a member of a group (referred to by "we") and the sentence may be uttered in the context of either the group's or the individual's deliberation. Like the singular sentence, the plural sentence may be uttered privately, that is,



addressed by the speaker to himself. When it is uttered publicly, it is addressed either to the group as a whole or to individual members of the group.

But "We ought to do X" may not be uttered in a context of deliberation at all. Furthermore, the addressee may be neither the group as a whole nor any member of the group. The sentence may be addressed to an outside party, and in that case the speaker is expressing an evaluation of act X as the best thing for the group to do. He is telling someone who is not in the group what the group ought to do. He is not telling the group what *it* ought to do.

Suppose, in a third kind of context, the group is the addressee but, as in the second kind of context, it is not involved in deliberation. Then the speaker as a member of the group is telling the group what *it* ought to do and so is *prescribing* the doing of act X to the group (assuming that the four necessary conditions for prescribing, to be stated below, are fulfilled)

I turn now to the type of "ought" sentence to which philosophers have paid most attention - second person *ante eventum* sentences ("You ought to do X"). These sentences are a form of direct address, the addressee being identical with the agent designated in the sentence. When such a sentence is uttered under the following conditions, its utterance is an act of prescribing :

(1) The sentence is uttered in earnest and is affirmed by the speaker.

(2) The addressee is an agent in a present situation of choice (or will be an agent in a future situation of choice) in which doing X is one of the alternatives.

(3) The agent (or addressee) has (or will have) the freedom to choose to do X or not to do X.

(4) It is considered by the speaker to be legitimate and proper for the addressee to demand reasons of the speaker as to why he, the addressee, ought to do X.

It should be noted that whenever the addressee and the agent of an *ante eventum* "ought" sentence are identical, the act of uttering the sentence under these conditions is an act of prescribing. Thus first person plural sentences are prescriptive when they are addressed to the group designated by "we" in the sentence, and (with the qualifications in mind that were set forth above) all first person sentences, singular and plural, are prescriptive when addressed by the speaker to himself. We shall see later that there are other kinds of sentences than "ought" sentences which may be used prescriptively, but in every instance

the four conditions listed must hold. I shall now briefly examine each condition.

(1) By saying that a sentence is uttered in earnest I mean to exclude not only its being uttered in jest, but also its being uttered simply to frighten, amuse, annoy, bewilder, shock, or have some other emotional effect upon the addressee. Indeed, a sentence would not have the capacity to bring about such effects if it were not normally used "in earnest". These emotional functions of a sentence are secondary or derivative. What, then, is the primary or non-derivative use of a sentence? It is the use of the sentence "in earnest", that is to say, the use of the sentence when the speaker's main intention in uttering it is to have the addressee give his sincere assent to what is being said. When uttering the sentence is an act of prescribing, the addressee's giving his sincere assent will involve at least his setting himself to do the prescribed act and his having a pro-attitude toward doing it.

To utter a sentence in earnest in this sense precludes its being uttered in certain special contexts. For example, a sentence is not uttered in earnest in this sense when it occurs in the context of poetry or fiction. Nor is it uttered in earnest in this sense when we are interested only in setting forth a proposition for consideration, or as a supposition. We do not utter a sentence in earnest in this sense when we are "mentioning" it but not "using" it. And it is not uttered in earnest in this sense when our sole purpose is to let the addressee know what we think about something, without expecting him to agree with us.

Now it is possible to utter a sentence in earnest and yet not affirm what it says. For example, we may not ourselves believe what we are saying, although we want and expect the addressee to believe it. This would be the case whenever we are lying to the addressee, or whenever we are trying to conceal from him our true thoughts and feelings. An "ought" sentence is prescriptive, then, only if it is both uttered in earnest and affirmed by the speaker. Throughout the foregoing discussion of "ought" sentences I have been assuming that sentences are being used in earnest and are being affirmed in the senses just stipulated. I shall continue to make this assumption for the remainder of the paper.

(2) The second condition, that the addressee be an agent in a situation of choice, is clear from what has been said earlier. To prescribe is to tell someone what he ought to do, and what he ought to do is the act which is the best alternative open to him. His situation of choice need not occur at the time when the act is prescribed, since it makes perfectly good sense to tell a person

that he ought to do a certain act in the future when he will have the choice of doing it or not doing it, even though that choice is not open to him in the present. There must be, however, a specific expectation that the future situation of choice will occur. Thus if we say "You ought to return the book you have borrowed," the addressee must either be in a present situation such that he can choose to return the book or not, or else there must be a specific future situation in which it is foreseen that he will have such a choice. Otherwise there would be no reason for our addressing the sentence to him. If, for example, he replies, "The book is lost I have looked everywhere and can't find it", we shift our prescription to: "Then you ought to replace it." We do not continue to prescribe his returning the particular copy of the book which has been lost, on the mere chance that it might be found in the unforeseeable future.

(3) The second and third conditions in conjunction may be summed up in the familiar "'Ought' implies 'can'". The third condition itself may be analysed into the following two requirements: (a) The agent will not do X unless he chooses to do X. (b) The agent will not choose to do X unless he decides to do so either as a result of his own deliberation or as a result of his freely given decision to follow another's deliberation. Both of these requirements presuppose that there are genuine alternatives open to the agent, which means first, that the agent has the physical ability and psychological (intellectual and emotional) capacity to do any of the acts in question; second, that the agent is not under external constraint (coercion, duress) to do one thing rather than another; and third, that the agent is not under the influence of any internal compulsion to do one thing rather than another. It makes no sense to say that someone ought to do something either when he cannot possibly do it or when he cannot help but do it. This is also true of *post eventum* "ought" sentences. To say that a person ought to have done X contextually implies that he did not do X but that he could have done X if he had so chosen and that he could have so chosen. Similarly, to say that a person ought not to have done Y contextually implies that he did Y but that he would have done otherwise if he had so chosen and that he could have so chosen.

To prescribe an act to someone is clearly different from forcing or compelling him to do it. Prescribing can occur only if the person is free to choose not to do the act prescribed. This condition derives from the fact that prescribing is one way of giving advice, making a recommendation, or offering guidance, and all of these activities presuppose that the person who receives

the advice, recommendation, or guidance is free to choose not to follow it. This also distinguishes prescribing from commanding, ordering, and issuing directives. When a person is in a position to be commanded or ordered to do something, he must obey (under the external constraint of a penalty for disobedience). But a person does not *obey* a prescription. He *decides* to follow it or carry it out; he *adopts* it as a guide to his conduct.

(4) It is because an act of prescribing is always an act of guiding someone's conduct (or the making of a recommendation, or the giving of a piece of advice) that the fourth condition must hold whenever prescribing takes place. This is the condition that it is always legitimate and proper for the person to whom one prescribes (i.e. the addressee) to demand reasons for his doing the prescribed act. The addressee's question "Why ought I to do X?" is never out of place. Prescribing, like all advising, recommending, and guiding, is a rational act; it presupposes its own justifiability. The person who prescribes may not be able to give a rational justification for his prescription, and the addressee may not *in fact* demand that he do so. But he who prescribes must acknowledge the *right* of the addressee to make such a demand. The person who is commanded to do something, on the other hand, is not in the position to ask why he should obey. He is engaged in a social practice whose defining rules are such that the person who commands him has authority over him. To demand a justification for obeying the commands is to place oneself *outside* that social practice and ask that it be justified as a *whole*. But a person may be engaged in the social practice of receiving advice, recommendations, or guidance and *within* the practice demand that the advice, recommendations, or guidance which he receives be justified. This is part of what it means to engage in such a practice.

In his article "Goading and Guiding" (*MIND*, lxii (1953), pp. 145-171) Professor W. D. Falk has given a cogent and detailed argument to show that all advising or guiding is in this sense rational. He points out that the purpose of advice or guidance is to tell a person what would be best for *him* to do. That person can always challenge the advice by challenging the reasons given (or presupposed) by the adviser. Whatever reasons are good reasons for the adviser must also be good reasons for the person being advised. (*Loc. cit.* pp. 161, 169-171.) But the fact that prescribing is a rational act in this sense does not mean that to prescribe an act to someone is to give him a reason for doing the act. Prescribing is not giving reasons, even though it presupposes

(contextually implies) that reasons can be given for doing what is prescribed. To utter a prescriptive "ought" sentence is to tell a person *what* he ought to do, not *why* he ought to do it. But it is also to claim *that* he ought to do the act in light of, or on the basis of, certain reasons. In short, prescribing is a rational act but not an act of reasoning. I shall show in Part II that this is a key to our correctly understanding the logical connection between prescribing and evaluating.

I turn now to universal "ought" sentences. These may take either an active form ("One ought to do X in circumstances C") or a passive form ("X ought to be done in circumstances C"), and I consider these two forms to be equivalent in meaning. To say "One ought to keep one's promises" is to say "Promises ought to be kept". Whatever assertions I make about universal sentences, therefore, will apply equally to these two forms. In universal sentences no particular agent is specified, although an act is specified (act X) and the circumstances in which the act ought to be done are specified (circumstances C). When such a sentence is uttered the addressee may or may not be in circumstances C and may or may not be able to do X. If he is not in circumstances C or if he cannot do X, the sentence does not apply to him. It does not function as a guide to his conduct, since no inference can be made from it about what he ought to do in the situation in which he is being addressed. Uttering a universal sentence is an act of prescribing only if either of the following conditions holds: (1) The addressee is in circumstances C and has the choice of doing or not doing X at the time when the sentence is uttered. (2) There is a specific expectation that the addressee will be in circumstances C and will have the choice of doing or not doing X in the foreseeable future. For suppose neither of these conditions were fulfilled. The speaker would not then be telling a person what *he* ought to do by addressing that person himself. He would instead be telling a person what *anyone* ought to do in certain circumstances. The speaker, in short, would not be prescribing but *stating a general rule of conduct*.

Sometimes rules of conduct are called "universal prescriptions", and one of the dictionary definitions of "prescribe" is "to lay down a rule". I concede that common usage of the terms "prescribe" and "prescription" allow for their being applied to rules, so I should not dispute with someone who wished to call a rule a prescription and the act of stating a rule an act of prescribing. Nevertheless I should want to make clear the difference between making a statement about how people in

general ought to act and telling a person how *he* ought to act. We might justify the latter (a particular prescription) by appeal to the former (the universal prescription of which the particular prescription is held to be an instance). Thus to state a rule of conduct would be to give a reason for doing an act. Prescribing a particular act to a particular person, on the other hand, is not to give a reason for his doing it. It is merely to tell him that he ought to do it. So laying down a rule is not, in *this* sense, an act of prescribing, unless the rule applies specifically to the addressee as an agent in a present or expected future situation in which he has the choice of following the rule or breaking it. When in this manner the rule is applicable to him, it not only tells him what to do but also gives him a reason for doing it. This does not mean that he cannot properly challenge the rule itself as a valid (or good) reason for doing the act. To justify an act by appeal to a rule leaves open the question of the justifiability of the rule itself.

In investigating the nature of prescribing I have been concerned so far only with the use of "ought" sentences. May not other kinds of sentences be used for this purpose? I think the answer to this is clearly in the affirmative. There are a number of other linguistic forms ordinarily used for prescribing, that is, for giving a direct and unequivocal answer to the question, "What should (ought, shall) I do?" In the first place there are imperatives. To claim that imperatives may function as prescriptions is to deny that all imperatives express commands or orders, in light of the distinction I have drawn between prescribing on the one hand and commanding, ordering, and issuing directives on the other. Yet it appears to me that sentences in the imperative mood can be used in these two different ways. Surely it is appropriate to answer the question "What should I do?" with such sentences as: "Do X." "Be sure to do X." "Do not fail to do X." This is one normal use for imperatives, so I take it that imperatives may express recommendations as well as commands. Which they express depends on the practical context in which they are uttered.

A second way to prescribe without using an "ought" sentence is by means of what may be called "necessity" words. Thus to say "You are obliged to do X", "You must do X", or simply "You are to do X" is certainly to give a clear and direct answer to the query, "What should I do?" Such "necessity" sentences are practically interchangeable with second person *ante eventum* "ought" sentences in the context of giving advice, making recommendations, or offering guidance.

Thirdly, there are a variety of expressions that are milder in their feeling-tone than "ought" sentences, imperatives, and "necessity" sentences, and yet which may be used prescriptively. Examples are: "I suggest that you do X." "My advice is. Do X." "I recommend that you do X." "If I were you, I should do X." In all such cases the four conditions I have specified as necessary for an act of prescribing to take place must be fulfilled. That is, none of these expressions is prescriptive unless (1) it is uttered in earnest and affirmed by the speaker, (2) the addressee is or will be an agent in a present (future) situation of choice in which doing X is one of the alternatives, (3) the addressee is or will be free to choose to do or not to do X, and (4) it is legitimate and proper for the addressee to demand reasons for his doing X. Indeed, these four conditions must hold if any of the other linguistic forms I have mentioned is to be prescriptive.

There is still another kind of expression that is appropriate for answering the question "What should I do?" This is the statement that one of the acts open to the questioner is the best thing for him to do. Such an answer, however, is not so much an act of prescribing (i.e. telling the person that he ought to do a certain act) as it is the giving of a (sufficient) reason for his doing the act. It is a direct answer to the question "Why should I do this?" One can always answer this question by saying: "Because it is the best thing to do." One cannot answer with a prescription: "Because you ought to do it", since the questioner already knows what he has been told he ought to do when he asks the question. Moreover, if the questioner goes on to demand reasons for the claim that a certain act is the best thing to do, we cannot reply with a prescription. In other words, whereas we may justify a prescription on the ground of an evaluation, we may not justify an evaluation on the ground of a prescription. For a prescription does not provide a *reason* for or against doing something. But this is just what an evaluation of an act does provide. It is my task in Part II to show why this is so.

## II

In every prescription it is necessary to distinguish the act of prescribing from the act prescribed. The thesis I shall try to establish is a double one: first, that *both* of these acts are justified on the basis of evaluations, and second, that the justification of the

act prescribed is a part of, but not the whole of, the justification of the act of prescribing.

When we *assert*, in a third person *ante eventum* "ought" sentence, that someone ought to do a certain act, we express an evaluation of the act as the best of the alternatives confronting the person. When we *tell* someone, in a second person *ante eventum* "ought" sentence (or in a sentence of one of the other kinds mentioned at the end of Part I), that he ought to do a certain act, our uttering the sentence is an act of prescribing. Now so far as the agent is concerned, his doing the prescribed act is justified only when it can be shown to be the best thing for him to do. It is not justified to him merely because it is prescribed to him. *Prescribing is not itself a way of justifying what is prescribed.* Why is this so? The answer lies in the fact that the relation between the act of prescribing and the act prescribed is like the relation between the act of uttering a sentence and what the sentence is about. For the act prescribed is part of the content of the prescription; it belongs to what is being said, not to the saying of it. The act of prescribing, on the other hand, is a linguistic act. As we saw in Part I, to prescribe is to utter a certain sort of sentence in a certain sort of circumstance. We cannot prescribe an act to someone unless we *tell* him what he ought to do. We cannot even prescribe to ourselves unless we speak to ourselves. Every act of prescribing is accordingly a form of direct address: he who prescribes must be in the position of a speaker and he to whom one prescribes must be in the position of an addressee. *What* is prescribed, however, may or may not be a linguistic act. ("You ought to tell him you are sorry" and "You ought to pay back the debt" are cases in which the act prescribed is, respectively, a linguistic act and a non-linguistic act.) And just as the saying of something is not giving a reason for believing what is said (except in such "degenerate" cases as "I am alive", "I can speak English", "I am not asleep", etc.), so an act of prescribing is not giving a reason for doing what is prescribed. The act of prescribing tells a person what he is to do; it does not tell him why he is to do it.

If the doing of an act is to be justified to a person in a situation of choice, the act must be shown to be the best alternative open to him. Showing him what is the best alternative requires an evaluation of the various alternatives. Let us suppose that this evaluation has been carried out and that the judgment: Act X is the best thing to do, has been reached. To pronounce this judgment in the presence of the person is to give him a (sufficient) reason for doing X. Let us then suppose that act X is prescribed



to him. In that case the pronouncing of the judgment (understood as the outcome of the process of evaluation) justifies the act prescribed.

Does it also justify the act of prescribing? My answer is that it justifies it in part, but not wholly. I have said that the doing of the act prescribed is justified when it is shown to be better than the doing of any alternative act. The act of prescribing is justified in the same way. But here we must distinguish two questions. (1) Is it better to prescribe act X than to prescribe any other act open to the addressee? (2) Is it better to prescribe act X at this time, in this place, in this manner, under these conditions, than at any other time, in any other place, in any other manner, under any other conditions? Question (1) concerns the content of the prescription, question (2) concerns the characteristics and circumstances of the act of prescribing. To justify completely the act of prescribing requires that we answer both questions in the affirmative.

The first question is answered in the affirmative when the act prescribed is shown to be the best of the alternatives open to the addressee. Now to justify the act prescribed in this way is partly to justify the act of prescribing it. For prescribing any other act than the best would amount to prescribing an act which the addressee was less justified in doing than some other act, *and such a prescription would not be the best possible prescription open to the prescriber*. I am assuming here that the purpose of prescribing is to have the addressee give his sincere assent to the prescription and accordingly set himself to do the act prescribed, or at least have a pro-attitude toward his doing it. Corresponding to each of the alternatives open to the addressee (let us say the alternatives are acts X, Y, and Z) there is an alternative prescription open to the prescriber (for example "You ought to do X", "You ought to do Y", "You ought to do Z"). A complete justification of the act of prescribing would have to include reasons showing that the prescribed act was the best. Otherwise a person would be justified in prescribing an act, and so influencing an agent to do an act, which the agent was not justified in doing. In short, if act X is better than acts Y or Z, then *ceteris paribus* prescribing the doing of X is better than prescribing the doing of Y or Z.

Alternative acts of prescribing may vary in other respects than their content (*i. e.* in respect of the acts prescribed in them). The alternatives open to a prescriber concern not only what is prescribed but also the time, the place, the manner (including the linguistic form of a prescription), and the surrounding conditions

of the act of prescribing. An act of prescribing is fully justified only if it can be shown to be the best of the alternatives open to the prescriber in all these respects as well as being the act of prescribing the best alternative open to the addressee. To decide on the best time, place, manner, and circumstance for prescribing requires an evaluation of alternative times, places, manners, and circumstances open to the prescriber. Even when he has decided upon what act to prescribe to the agent, the prescriber must then come to a decision about these other matters in order to arrive at the best prescription available to him. And it is only if his prescription is the best available to him that it is justified (both to himself and to the agent).

Nothing in principle differentiates the evaluation of the act prescribed from the evaluation of the time, place, manner, and circumstance of the act of prescribing. Both evaluations presuppose some standard or set of standards being appealed to. Both are made within a given class of comparison (in the one case, the class of acts open to the agent; in the other, the class of acts of prescribing open to the prescriber). Both consist in the grading or ranking of the alternatives in an order of desirability, the best alternative being the most desirable or least undesirable member of the class of comparison. The desirability or undesirability of each alternative is determined by its good-making and bad-making characteristics, as defined in accordance with the standards appealed to. One type of standard appealed to is whether the act fulfils or violates a rule of conduct ("universal prescription") It is in this way that a rule of conduct functions as a *reason* for or against doing an act. (When this type of standard is appealed to, it would be more accurate to speak of "right-making" and "wrong-making" characteristics than of "good-making" and "bad-making" characteristics.)

An act of prescribing is justified on the grounds of these evaluations. The evaluations are themselves justified only if (1) there are good reasons for claiming that the alternatives have the good-making and bad-making characteristics attributed to them, and (2) there are good reasons for accepting the standards appealed to as appropriate to evaluations of this sort. The question of what constitutes good reasons in (1) and (2) is a question beyond the scope of this paper.

I should like to add one final point. The evaluation of the act prescribed and the evaluation of the time, place, manner, and circumstance of the act of prescribing together form the sufficient conditions for a complete justification of an act of prescribing. For all the possible ways in which acts of prescribing can differ

are covered by these evaluations. The evaluations consequently exhaust all the possible alternatives open to a prescriber's choice. Since the most justified prescription is the result of choosing the best alternative, no further justification need be given once these evaluations are themselves justified. Indeed, no further justification would be possible.

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## VI.—DISCUSSIONS

### A SYSTEM OF DEONTIC-ALETHIC MODAL LOGIC

1. *Introduction.* In recent years several systems of deontic logic have been published, and some attempts have been made to link deontic systems with some of the well-known alethic systems. In this paper I shall present a very simple method of doing so, which also provides a simplification of the deontic system in an important direction.

The basic idea<sup>1</sup> of the system I shall present is this. In this system, hereafter referred to as the system O, the variables are imperative, not propositional, when unmodalised, and propositional, or quasi-propositional, when modalised. The meaning and justification of this departure will be given in the next section. The system O differs further from all published systems in that it is formally identical with the alethic modal system S5. In all published systems *COPp* is rejected, and is replaced as an axiom by *COpNONp*, i.e. *COpPp*. In the system O the corresponding axiom is *COpPp*. (The meaning of the dot is explained in the next section.) One advantage of this system is therefore that all known results about S5 hold for it also, though some of them fail when S5 is weakened by having *COPp* replaced by *COpNONp*. This constitutes a useful simplification of deontic logic.<sup>2</sup>

The system is offered as one which has some of the properties necessary to one which is to be used to formalize the logical properties of moral discourse.

2. *Imperatives.* Some interest has recently been taken in the formal logic of imperatives. I shall not be concerned here with the details of any formal system, but only with the arguments which support my contention that propositional variables may be given an alternative interpretation.

The best-known account of this position is given by Hare.<sup>3</sup> He offers an analysis of imperative sentences into two parts, which he calls and I shall call neustic and phrastic. The phrastic refers to, and according to Hare describes, a "state of affairs"; the neustic is a kind of flag used to indicate, to put it roughly, what the speaker or writer is saying about that state of affairs. In Hare's explicitly simplified terminology, when the sentence is indicative the neustic indicates that the state of affairs is being said to obtain, while when

<sup>1</sup> I owe this idea to Mr. R. M. Hare, to whom I am grateful for helpful comments. Mr. E. J. Lemmon pointed out several mistakes in earlier versions and has greatly helped me to clarify the present one by his sceptical comments. Professor H. Törnblom suggested several improvements in the present formulation.

<sup>2</sup> There is a full bibliography in A. R. Anderson, *The Formal Analysis of Normative Systems*, Yale Interaction Laboratory, New Haven, Connecticut, 1956.

<sup>3</sup> R. M. Hare, *The Language of Morals*, ch. 2, Oxford University Press, 1952.

it is imperative the state of affairs is being commanded. Hare's method of writing out his analysis of a sentence is to write a noun-phrase, referring to the state of affairs, inside single brackets, and to add either "yes" or "please" to the right of the final bracket. What appears inside the brackets is called the phrastic (part) of the sentence, what appears outside them its neustic (part). For example, "(Your closing of the door in the immediate future) please" is the analysis of "Close the door", while "(Your closing of the door in the immediate future) yes" is the analysis of "You are going to close the door".

I here accept Hare's thesis that imperative sentences, provided of course they are in use, can be analysed into phrastic and neustic. But I shall use a different and more convenient symbolism. I use the usual  $p, q, r, p_1$ , as variables, and I interpret them as phrastic variables. That is, their range of permitted substituents is the set of expressions which can be or are normally used to write down a phrastic. Further, I shall omit any symbol for the indicative neustic. The imperative neustic I show by means of a dot placed above the phrastic-variable, thus  $\dot{p}$ . It is possible to omit the indicative neustic without ambiguity because in the contexts I shall be considering phrastics never appear without an accompanying neustic.

In what follows I use the bracket-free notation of Łukasiewicz, capitals  $A, B, C, A_1$ , as syntactical variables (range of permitted substituents arbitrary expressions of the system), and as syntactical notation I use " $\vdash A$ " to mean " $A$  is a theorem of the system under consideration" and " $A \rightarrow B$ " to express the rule that  $B$  may be inferred from  $A$ .

To fix our ideas about imperative logic, let us try to answer the following questions. What, if anything, in imperative logic corresponds to the rule of detachment in indicative logic, i.e. to the rule " $\vdash A$  and  $\vdash CAB \rightarrow \vdash B$ "? And, if we can find an analogue, how are we to interpret it? To answer these questions we shall have to decide what is to correspond in imperative logic to the familiar notions of validity and truth.

Corresponding to the notion of truth we have in imperative logic the notion of being obeyed (or being fulfilled)<sup>1</sup>. There are many considerations which point to this fact, perhaps the most important is this. Just as someone can be said to understand a statement if and only if he knows in what circumstances it would be true, so also someone can be said to understand a command if and only if he knows in what circumstances it would be obeyed—not those in which anyone would be prepared (willing) to obey it, but those which would count as its being obeyed. If this is accepted, a definition of validity is available. A valid argument is one in which the fact that the premisses are obeyed guarantees that the conclusion

<sup>1</sup> For an early treatment of imperative logic from this point of view see A. Ross, "Imperatives and Logic", *Philosophy of Science*, ix (1944), 30-46

is obeyed. And in general a valid—or “logically obeyed”—command is one which is obeyed no matter which of its component commands are obeyed or disobeyed. Or in terms of obedience-values, which we can think of as analogous to truth-values, a command is valid if and only if it takes the obedience-value “obeyed” (referred to as O) for all assignments of the obedience-values O and D (disobeyed) to its variables.

It is now possible to give a matrix definition of the connectives used in imperative logic. The command  $Np$  is obeyed if and only if the command  $p$  is disobeyed (or, as I prefer to say, not obeyed, since disobeyed commands are not just commands which are not obeyed); and the command  $Kpq$  is obeyed if and only if the command  $p$  is obeyed and the command  $q$  is obeyed. We then find, however, that the resulting matrix is the same as that characteristic of the classical propositional calculus. So there is no need to distinguish between the connectives used in imperative and those used in indicative logic. We need only remember that each value, truth- or obedience-value as the case may be, is determined by the same matrix, with either O or T (designated value) and either D or F (undesignated value). Combined indicative-imperative logic presents a few further problems, such as the question whether  $Cpq$  is to be taken as an indicative or an imperative, how  $Cpq$  is to be interpreted, and so on; these present no real difficulty and will not concern us further.

A rule corresponding to the rule of detachment in indicative logic is now available for imperative logic:  $\vdash A$  and  $\vdash CAB \rightarrow \vdash B$ .

I shall not develop imperative logic any further here. Apart from the small points of interpretation mentioned it is only trivially different from indicative logic. What is of interest from a formal point of view in this field is the logic of commanding, and of asserting — i.e. of statements that someone commands or asserts something. I hope to discuss it elsewhere.<sup>1</sup>

3 *Alethic modal logic* I give here for reference purposes, a formulation of the alethic modal system S5; I shall, however, refer to it as the system L. The formulation is due to Gödel.

#### Elements

(a) Primitive symbols:  $N C L$

(b) Variables:  $p, q, r, p_1, \dots$  an infinite list

#### Rules of Formation

F.1. A variable standing alone is a well-formed formula (wff).

F.2. If A and B are wffs, so are  $NA$ ,  $CAB$ ,  $LA$ .

#### Axioms

(PC) Every theorem of classical propositional calculus.

(L.1)  $CLpp$

(L.2)  $CLCpqCLpLq$

(L.200)  $CLNpLNLp$

<sup>1</sup> See “A logical theory of commanding” forthcoming in *Logique et Analyse*.

*Rules of Inference*

R 1.  $\vdash A \rightarrow \vdash A_1$ , where  $A_1$  results from  $A$  by the substitution of any wff (any  $n$  wffs) for each occurrence of some variable (some  $m$  variables) in  $A$ .

R 2.  $\vdash A$  and  $\vdash CAB \rightarrow \vdash B$ .

R 3.  $\vdash A \rightarrow \vdash LA$ .

4. *Deontic logic* The system  $O$  is identical with the system  $L$  except that the variables of  $L$  are replaced systematically by the dotted variables  $\dot{p}, \dot{q}, \dot{r}, \dot{p}_1, \dots$  of  $\dot{O}$ . (It is intuitively obvious, I hope, what is meant by each variable in either system corresponding to just one variable of the other system);  $L$  is replaced by  $\dot{O}$ , and the axioms are re-numbered (0.1), (0.2), (0.200)

Although the system is formally identical with  $L$ , its interpretation ensures that it is nevertheless of some independent interest. The intended interpretation is as follows. The variables  $\dot{p}, \dot{q}, \dot{r}, \dot{p}_1, \dots$  are imperative variables; their range of permitted substituents is, not the set of sentences normally used to make statements, but the set of sentences normally used to give commands. I here assume, with Hare<sup>1</sup> that whenever sentences in the imperative mood are used, whether to give commands, issue instructions, tender advice, or plead for help, there is a common core of activity which can be labelled "giving commands". Next,  $Op$  is interpreted as a judgement of obligation; it should be read "it ought to be the case that  $p$ " (here  $p$  is the indicative sentence), or " $p$  ought to be the case" (here  $p$  is the phrastic).

As I have indicated, the novel feature of  $O$  interpreted in this way is the fact that it is a deontic logic which has  $COpp$  as an axiom.  $CpNONp$ , i.e.  $CpPp$ , is therefore provable. In previous systems of deontic logic  $COpp$  and  $CpNONp$  had undesirable interpretations (or, as in Von Wright's, none), and were rejected, at least in those systems which had a suitable matrix.  $COpp$ , for instance, has commonly been interpreted as "If it is obligatory that  $p$  (be the case) then  $p$  (is the case)", which clearly is not a logical truth. But the interpretation intended of  $COpp$ , namely "If it ought to be the case that  $p$ , then  $\dot{p}$ ", is a logical truth; at least, I am putting it forward as one. My justification is that, in a perfectly clear sense, it would be inconsistent to pass the judgement that  $p$  ought to be the case while refusing to give the command to make  $p$  the case. If it is objected that this is not logical inconsistency, my reply is that, from the purely extensional point of view, it is not logically inconsistent to assert that something is necessarily the case while denying that it is the case, but this is not usually urged as an objection to the alethic axiom  $CLpp$ , since it is admitted by all that alethic modal logic is not purely extensional. Nor, I suppose, is the system  $O$ , since it is formally identical with the non-extensional system  $L$ . My suggestion is that the inconsistency in passing the judgement while

<sup>1</sup> R. M. Hare, *op. cit.* p. 4.

refusing to give the command is the same kind of inconsistency as that involved in making the apodictic statement while refusing to make the assertoric one. If the latter is logical inconsistency, no doubt in a slightly extended sense, so too is the former.

The gain has so far been one of formal simplification. This is of course desirable for its own sake; moreover, recent attempts to define normative concepts in terms of alethic modal and purely logical ones having failed, as I think Lemmon and Nowell-Smith have conclusively shown,<sup>1</sup> the present simplification may be seen, perhaps, as a less ambitious move in the same direction. The result has so far been to make deontic logic of slightly less formal interest than it was before. However, the systems L and O can be combined in a way which provides a slight but genuine extension of each.

5. *Alethic-deontic logic.* The system LO will now be formally presented.

### *Elements*

The elements of the systems L and O are elements.

### *Rules of formation*

F.1. The wffs of the systems L and O are wff.

F.2 If A and B are wffs, so is  $CAB$ .

### *Axioms*

(PC), (L.1), (L.2), (L.200), (O.1), (O.2), (O.200), as given for the systems L and O,

(LO.3)  $CpNLNp$ .

### *Rules of inference*

R.1. (a) Same rules as in the systems L and O, provided that only either  $p, q, r, p_1, \dots$ , or  $\hat{p}, \hat{q}, \hat{r}, \hat{p}_1, \dots$  are variables in A,

(b)  $\vdash A \rightarrow \vdash A_1$ , where  $A_1$  is the result of replacing all occurrences in A of a variable of the system L (an L-variable) by any wff B containing only L-variables, and all occurrences of the corresponding O-variable by the wff C which is the result of replacing all the L-variables in B by the corresponding O-variables, or of replacing any  $n$  variables in A by any  $m$  such wffs,

(c) If A is a theorem of the classical propositional calculus, then  $\vdash A_1$ , where  $A_1$  is the result of replacing each occurrence in A of an L-variable by the corresponding O-variable, or replacing any  $n$  such L-variables by the corresponding O-variables

R.2. Same rule as in the systems L and O.

R.3. The rules R.3. of the systems L and O.

<sup>1</sup> E. J. Lemmon and P. Nowell-Smith, "Escapism - the logical basis of ethics", *MIND*, LXX (1960), 289-300, this is directed against Prior's essay of the same title, and Anderson *op. cit.*



The rule of substitution R 1. is complicated, but its effect is quite simple. It allows substitution on variables of each sub-system, provided that the corresponding substitution is made on the corresponding variable of the other sub-system if it is present, but if the formula in which substitution is to be made is a theorem of propositional calculus then dots may be introduced on each occurrence of any variable occurring in it. The considerations which were adduced in section 2 should make the reasons for this rule obvious.

The special axiom (LO.3) is, I suggest, a logical truth in just the same sense as that in which  $COpp$  is one, and informal justification of it would be exactly similar. I have adopted it as the strongest simple axiom connecting the two systems which I can find. There may well be others which are independent of it and also logically true, which they are I leave an open question.

The following is a list of theorems which can be simply proved in LO. I omit the proofs, which are elementary in all cases.  $M$  abbreviates  $NLN$  and  $P$  abbreviates  $NON$ , as usual.

(LO.4) $COpNLNp$	(LO 15) $CCpqMCpq$
(LO.5) $CNMpNOp$	(LO.16) $COCPqMCpq$
(LO.6) $CNMpNOp$	(LO.17) $CCpqCLpMq$
(LO.7) $COpCNMpq$	(LO.18) $COCPqCLpMq$
(LO.8) $COpCNMpq$	(LO 19) $CNMCpqNCPpOq$
(LO.9) $CNMpCpq$	(LO 20) $CNMCpqNCPpOq$
(LO.10) $CNMpCpq$	(LO 21) $CNMCpqNCPpOq$
(LO.11) $CpCNMpq$	(LO.22) $CLCPqCOpPp$
(LO.12) $CpCNMpq$	(LO 23) $CNFCpqNCMpLq$
and various other "paradoxes"	
(LO.13) $CLpp$	(LO 24) $CNCpqNCMpLq$
(LO.14) $CLpPp$	(LO 25) $CNCpqKMpMNq$
	(LO.26) $CONCpqKMpMNq$

6. *Further developments.* Quantifiers could now be introduced. A two-sorted functional calculus would have to be used, with one sort of variable having expressions referring to persons as permitted substituents and the other sort having expressions referring to situations (occasions). As I have suggested, an eventual aim of this development would be a definition of  $O$  (of normative and moral concepts) in terms of imperatives and quantification, so that  $Op$  would be displayed as some appropriate universally quantified imperative

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## KNOWING AND UNDERSTANDING

We speak of understanding why something is the case, how a trick is done, when to keep silent, what a person is saying or doing and so on. and a question can be raised as to what, if anything, these seemingly different expressions have in common. In other words, it can be asked whether the various senses of "understand" have a family resemblance and, if so, what the family is.

One answer to this question is suggested in *The Concept of Mind* (pp. 51-60). Professor Ryle discusses that sense of understanding which is roughly equivalent to following what someone is saying or doing, and he argues that understanding, in this specific sense, is a "part of knowing how" because the "knowledge that is required for understanding intelligent performances of a specific kind is some degree of competence in performances of that kind". Now it may well be that Ryle does not intend to generalise from the specific sense he discusses to the concept as a whole, but there is certainly a temptation to do so since it is natural to assume that, if a concept has different senses, more or less systematic connections will hold among them,<sup>1</sup> and a family connection with "knowing how" ensures respectability.

Unfortunately, however, a close relationship with "knowing how" is not easy to establish for the more general senses of understanding why, how and so on. Imagine the case of a person who inquires why salt is scattered on the pavement in icy weather and is told that salt lowers the temperature at which water will freeze. If the person then understands, it is plausible to claim that this is because he has been given information which enables him to connect the matter with his existing knowledge. It is true, of course, that he now knows how to account for the scattering of salt, but this is only to say that he is able to make use of what he has been told and believes to be the case. Similarly, if the conjuror explains where he put the penny that disappeared, we understand because, having been told, we know that the penny went up his sleeve. It may be argued that we also know how to do the trick. But what does "knowing how to" mean in this case? It cannot amount to being able to do the trick since a person does not necessarily acquire the ability to do sleight-of-hand just from being shown how to do the trick, and, if "knowing how to" involves no more than knowing how the trick is done, it is a knowing *that* which is involved.

Analysis of these general senses of "understanding" seems to suggest, then, that the concept belongs to the knowing *that* family. Moreover, Ryle's specific sense can also be claimed as a relative. The ability to follow what a person is saying is indeed a skill acquired by imitation and practice. But this plausible piece of learning theory must not be allowed to displace the epistemological contention

<sup>1</sup> Cf. Aristotle, *Metaphysics*, 1003a21-b19.

that the skill depends on a person's knowledge. Indeed, facility in the exercise of this basic skill varies directly in proportion to a person's knowledge. To follow (or conduct) a conversation about servicing a motor-car engine a person must have knowledge of engines and the more he knows of them the better he will follow. And thus has general application to all communication. To use language with understanding it is necessary to possess concepts, and the possession of concepts, however it may govern our skills, is itself basically a function of our knowledge *that*. Similarly, our ability to understand what someone is doing when he plays trump depends on the extent to which we possess the relevant concepts, or, in other words, on our knowledge of the terms, rules and strategies of the game. Hence, it is at least arguable that Ryle's specific sense of "understand" is akin to the more general senses, since it too seems logically dependent on our knowledge *that*.

It may be objected here, however, that I am deceived by the "intellectualist legend" that before we can operate intelligently we must have planned theoretically, or that to understand a person's words and actions we must make inferences from overt performances to internal processes, or from our own internal processes to corresponding internal processes in others. But I do not think the claim that understanding is a form of knowing *that* and not of knowing *how* is "intellectualist," in this vicious sense. Moreover, I believe that any plausibility the charge may possess is based on a conflation of two distinctions, each of which has a *prima facie* acceptability. The conflation is of the distinction between knowing *how* and knowing *that*, and the distinction between the dispositional and the occurrent or episodic analyses of knowing.

The objection assumes that a person who claimed that understanding is a form of knowing *that* would also give an episodic analysis of knowing *that*. But this is not necessarily so. the claim that understanding is a form of knowing *that* is logically distinct from the claim that, when we understand, a series of occult episodes is occurring.<sup>1</sup> I cannot follow what a person is saying unless I know what his words and phrases mean, but I do not necessarily rehearse this knowledge internally while I listen, although I may do so if the language is one in which I am not fluent or the subject involves unfamiliar jargon. In a similar manner, a person may watch a game with understanding without inferring from overt moves to occult events in the minds of the players, but he cannot watch with understanding unless he knows what moves are standard or what relationship holds among the players in a team or what the object of the game is. It is true, as Ryle says, that he may pick up the rules or the standard moves without formal learning. But knowing *that* something is the case does not always involve formally learning it; not all knowledgeable people are scholars.

<sup>1</sup> A dispositional analysis of knowing *that* is developed by H. H. Price in *Thinking and Experience*

If knowing *that* admits of a dispositional analysis, however, it may be thought possible to assimilate it to knowing *how*. Mr J. Hartland-Swann, for example, claims that a person who knows that something is the case has the capacity to state correctly what is the case, and he argues from this premise to the conclusion that knowing *that* can be unpacked into knowing *how*.<sup>1</sup> As Hartland-Swann admits, however, "... many people will find it difficult to believe that 'knowing that the earth is round' is ultimately on a par with 'knowing how to swim,'" (p. 60). But there is a disparity, he thinks, only because different kinds of capacity are involved. The difference between the capacities is precisely this, however, that the second rests on a basis of muscular skills and reflexes whereas the first rests on a basis of knowledge that something is the case. In short, the first is not fundamentally an example knowing *how* but of knowing *that*, and to lump both together as "capacities" is to obscure this important difference.

Yet, even if the "knowing *that*" analysis of understanding neither entails the "intellectualist legend" nor reduces to a higher-order "knowing *how*" analysis, it has still to satisfy two other criteria mentioned by Ryle: it must be able to account for partial understanding and for misunderstanding.

Ryle points out that a person can be called "part-trained" but not "part-informed", and he uses this non-parallelism between the concept of knowing *how* and the concept of knowing *that* to support his account of understanding; for we can certainly have a partial understanding of something (p. 59). Ryle does note, however, that we can speak of a person as having partial knowledge "in the special sense of his having knowledge of a part of a body of facts or truths".

But it is this "special sense" of partial knowledge which is in fact relevant to an analysis of partial understanding. The knowledge involved in understanding is not just a knowledge of this or that, but a systematic knowledge. And if a full understanding of something thus involves knowing all about it, it seems reasonable to say that a partial understanding involves knowing a bit about it. A person with a partial understanding of an event or a discussion may know all about some aspects of it, or something about the whole of it, but his partial understanding is still a knowing *that*.

A similar account is possible for misunderstanding. A person misunderstands why something is so because of false or inadequate information. If he is to be enlightened, his information must be corrected or extended, and only when he has attained some degree of systematic knowledge can we say that he now understands the reason. So, too, misunderstanding of what was said or done requires for its cure corrected or elaborated information or the removal of false assumptions.

It should be noted in conclusion, that I am not denying that knowing *how* may be an aid to understanding: knowing *how* to swim, for example,

<sup>1</sup> *An Analysis of Knowledge*, p. 61.

machine may be a help in understanding how the machine works. Nor am I denying that understanding may often reveal itself as a knowing *how*: an understanding of the Highway Code may be revealed in skilful driving. My contention is rather that it is only in so far as knowing *how* can be analysed in terms of knowing *that* that understanding may be regarded as knowing *how*. But since, like Ryle, I do not think that knowing *how* can always be fully analysed into knowing *that*, I have argued that understanding is a branch of the knowing *that* family.

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## PROFESSOR BERLIN ON 'NEGATIVE FREEDOM'

PROFESSOR ISAIAH BERLIN, in his monograph *Two Concepts of Liberty*, defines *negative freedom* as being left "to do or be what one wants to do or be without interference by other persons". Three arguments are offered in defence of this definition.

Berlin argues, first, that freedom entails the absence of *coercion* or of *enslaving circumstances*. Both terms as ordinarily used entail the presence or absence of *deliberate human* interference (see p. 7). Therefore, freedom entails the presence or absence of *deliberate human* interference. But this argument is both misleading and beside the point.

One could just as easily, and just as correctly, point out that 'freedom', as ordinarily used, entails absence of *constraint* or *obstruction*. Both terms entail the presence or absence of interference, but not necessarily *human* interference. The abyss which prevents one from getting to his intended destination is a *constraint*, but not a form of *coercion*.

This merely points up the fact that discussion of ordinary use cuts no ice either way as far as this concept is concerned. For the problem is one of introducing a theoretical concept, 'negative freedom', which is suitable for theoretical consideration of those moral and political perplexities which generate the need for the expression in the first place. To the extent that we place a premium on communication, departure from ordinary meanings ought to be avoided. But discussion of use is a waste of time.

Berlin's second argument is that his definition corresponds to what representative liberal-utilitarian theorists have meant by freedom. It may or may not be true that Hobbes, Locke, Bentham, James Mill, and J. S. Mill (to mention only a few of the more important political philosophers that Berlin probably has in mind) used 'freedom' in a specified way. If they did, it may have been because their main concerns or problems were narrower ones than those we do or should consider today. This remains to be determined.

In fact, Berlin oversimplifies their use. While it is true that the liberal theorists he has in mind were primarily concerned with problems connected with the use and abuse of power, there is nothing in their theories which would have compelled them so to restrict their attention. Leonard Hobhouse argued this point very well in his little volume, *Liberalism*. It happens that the use and abuse of power was the problem of their age according to their moral and empirical lights. Hence, they tended to restrict their attention to problems involving *human* interference. But times have changed. Today obstructions to the human will which have little or nothing to do with the community's pattern of power relations have attracted the attention of liberal theorists.

This point brings us to Berlin's third argument. He argues that negative freedom is involved in the answer to the question, "What is the area within which the subject—a person or a group of persons—

is or should be left to do or be what he wants to do or be, without interference by other persons?" If this is the only question to which use of negative freedom is relevant, then Berlin's definition is entirely justified. The tacit claim that this is the only question does indeed underlie much that Berlin says in his monograph. But is this a legitimate presupposition?

Berlin is correct in arguing that concern with power lies at the heart of traditional liberalism, and that departing from this tradition is fraught with peril. The fact that in many places the abuse of power is no longer as obvious as it once was does not diminish the danger—quite the reverse. What those who rule, in a very wide sense of 'rule', were once able to achieve through the brutal use of instruments of force and violence, they are today often able to achieve through more subtle, but still barbarous, manipulation and use of organizational sanctions. The creation of great concentrations of economic, political, social, and even educational power today threaten essential liberties as much, if not more, than control of physical force once did in tyrannical monarchies. If anything, it seems to me that Berlin is not sufficiently radical in his exposure of these threats to our essential liberties.

But even admitting this, it is not at all clear that the problems which involve use of negative freedom end there. Traditional liberal concern with the problem of power developed within a world in which scarcity of material resources was the rule. No nation had achieved anything like the degree of material welfare which today exists in nations like the United States, Western Germany, England, France and the Soviet Union. Consequently, some traditional liberals came to accept the framework of scarce resources as given. From the point of view of constructing a political programme, they were not particularly concerned about those obstructions to the exercise of human will which were due to the scarcity of material resources. For them, the problem of poverty was a problem which had to be dealt with through thrift, hard work, philanthropy, and a tragic view of the human situation. For other traditional liberals, the problem of scarcity was one which would be solved automatically, once the mechanisms of a free market economy had been given a chance to function. Their policy aim was to eliminate interference with those mechanisms. Thus two groups of thought developed, one which was based on dismal pessimism, the other on a kind of patient optimism—neither thought positive governmental remedies possible or desirable. Eventually, some of these theorists came to accept the socialist insistence that the exercise of economic and political power was relevant, and was a proper object of social policy. But they stopped short of any concern with interference which could not be accounted for in terms of deliberate exercise of power.

Today an incredibly large material gap has opened between certain nations—for example, the United States and India. To focus

exclusively on power relations—on forms of deliberate *human* interference—is to conceal or deny the obligation that many feel Americans owe to Indians in virtue of this disparity. For Indians are made unfree by their poverty—though not unfree in Berlin's restricted sense. They lack material resources. This lack obstructs fulfilment of legitimate desires. But these obstructions are not due to any *deliberate* human interference in the affairs of Indians—unless failure to meet or even recognize an obligation is regarded as a form of deliberate human interference.

The crucial theoretical point can be put in the following way. The distinguishing principles of liberalism are human rights. The formulation of any liberal right essentially involves use of the concept, freedom (In Berlin's terms, negative freedom.) The basic moral issue is, "What is the proper scope of basic human rights? Should they be restricted to cover only human interferences? Or should they embrace forms of non-human and inadvertent interference as well?" Those who think that forms of non-human and inadvertent interference should be included within their scope will not be satisfied with Berlin's definition.

If the scope of rights is broadened in the way suggested, then important practical consequences would follow. For example, within a nation it might confer an obligation to develop a river valley so that floods are prevented and electrical power obtained. As between nations it would entail that provision of capital and food by a wealthy nation to a poor one would be, not simply a matter of prudence or charity, but of right and corresponding obligation.

There are passages in Berlin's monograph which seem to imply a much less restricted and more complex notion of negative freedom than the one I have attributed to him. For example, he argues that if someone who holds a social and economic theory which explains a person's lack of mental or physical capacity in terms of arrangements made by human beings, then he can appropriately describe the lack as an absence of negative freedom. But either this qualification does not dispense with the criterion of deliberate human interference, as ordinarily understood, or it makes Berlin's insistence on the deliberate human factor incomprehensible and morally trivial. But the entire thrust of Berlin's monograph conflicts with a trivial interpretation. That is, the significant restriction he seems to place on the meaning of 'negative freedom' coheres both with his "foxiness" and with his strongly critical attitude towards extension of state power. But I think also there is an element of profound confusion in this aspect of his analysis. My main aim has simply been to expose what Berlin's analysis tends to obscure in order to highlight the genuine moral alternatives involved in this conceptual choice.



## AQUINAS ON INFINITE REGRESS

A NOTE in a recent issue of *MIND* sees a *petitio principii* in the argument of St. Thomas Aquinas against an infinite series of movents. In that argument the observable movents are described as *moventia secunda*. But the serial order understood in the description "second" does not allow for the possibility of an infinite series of moved movents. The argument therefore presupposes what it undertakes to prove, namely, that there is a first movent in the series.<sup>1</sup>

No one would quarrel with this critique just in itself, absolved as it stands from any concrete background of place and time. But does it at all come to grips with the argument of Aquinas in the argument's own medieval setting? Is it not transferring the demonstration to a quite alien plane of abstract logic, a plane on which the argument was never meant to function? Most philosophers at the present time, as Ryle<sup>2</sup> has noted in one of his apt similes, would liken their activities to the work of the cartographer rather than to that of the detective. But was this the case with the medieval thinkers, and in particular with St. Thomas? Did Aquinas look upon metaphysics as plotting the contours of concepts, or as searching for some one who did something? The descriptions in places like the Proem to the *Commentary on the Metaphysics of Aristotle* shew definitely enough that he regarded metaphysics as a search for the ultimate causes of things, causes that at least in part existed separately from the material world. As a Christian he was convinced in advance that the primary movent of things was the God he accepted on faith. But, like the detective already psychologically convinced of the identity of the murderer, he was seeking metaphysical proof that would stand up in a philosophic courtroom. As clues, there lay before him sensible things and their real movement in the external world.

In the Aristotelian example, a stone was being pushed along by a cane, the cane in turn being propelled by a hand. Any other instance of something actually being moved would have served St. Thomas just as well. Aristotle's (*Ph.*, III 1,200b25 ff.) analysis had shown that any movement is of its very nature in potency to something further. Therefore, Aquinas (*In VII Phys.*, lect. 1, Leonine no. 6) interprets, movement is something essentially dependent. It cannot have a "first" or primary instance in its own order. This inherent imperfection gives anything affected by movement, and so any moved movent, a secondary status as a movent. Whether a particular series of moved movents is finite or infinite in number, is beside the point. Mathematical order does not enter into the reason for the status of these movents as secondary. Whether they are first

<sup>1</sup> C. F. J. Williams, "*Hic autem non est procedere in infinitum* . . ." *MIND*, lxxix (1960), 403.

<sup>2</sup> Gilbert Ryle, "The Theory of Meaning", in *British Philosophy in the Mid-Century*, ed. C. A. Mace (London, 1957), p. 264.

or third or fourth or millionth in serial order, or whether in an infinite series they have no serial number at all, they are all *moventia secunda* as St. Thomas understood the expression.

Whether the series of moved movents had a beginning in time or will have an end in time, is indifferent to the argument of Aquinas. Nor would he have anything against indefinitely continued substitution of one moved movent for another.<sup>1</sup> A man could have been driving the car that went into the ditch just as well as the woman. On the basis of the closing passages in *De Aeternitate Mundi*, St. Thomas would not see any philosophical objection to an actually infinite number of men and women on hand to take over in turn the wheel. The "endless alternatives of sufficient causes" (Williams, p. 403) do not in any way affect his argument. No matter how auto-mobile or even automotave the horseless carriage may be, it needs a chauffeur to make it go. The chauffeur decides freely to drive it, still according to St. Thomas,<sup>2</sup> his free will needs a mover of infinite efficacy to move it into act. The "microscopic" (Williams, p. 405) act of human free will requires a prior movent just as much as does necessitated movement. In fact, on account of its special indetermination, it requires the movent even more. Considerations like this show how different from the modern outlook is the approach of Aquinas to these problems. Yet his approach has to be respected if his argument is at all to be understood.

The result of the argument is that any movement in each and all of the moved movents, whether they are finite or infinite in number, has to be caused ultimately by a movent that remains unmoved in causing the motion. Because unmoved, it initiates movement absolutely and not in virtue of any prior movent. It may therefore be denominated "first movent". For Aristotle there were many such, for St. Thomas only one. That is a further issue, but it indicates profound difference between the approach of Aristotle and the approach of Aquinas. The point immediately decided by the argument now in question, however, is that any series—whether finite or infinite—of moved movents requires an unmoved movent that is outside their whole order. In the case of a mathematical series, the parallel would be that a mathematical succession, whether finite or infinite, has to be thought by a mathematician who, as the thinker, remains outside the whole order of the mathematical entities in the series.

The *Summa Theologiae* is of course just a *summa*. It restates arguments in summary fashion. In this particular instance the argument summarized is given at greater length in the earlier *Summa contra Gentiles* (I, 13), with references to its Aristotelian

<sup>1</sup> "Hence it is not impossible that man be engendered by man in infinitum" *Summa Theologiae*, I, 40, 2, ad 7m. The *Contra Gentiles*, I, 13 (ed. Leonine), v. XIII, p. 33b10, concedes that the argument is "most efficacious" when the perpetual series of moved movents, as understood by Aristotle, is admitted. Cf. *In VIII Phys.*, lect 2, Leonine no. 4.

<sup>2</sup> *Contra Gentiles*, III, 89; *Summa Theologiae*, I, 83, 1, ad 3m; I-II, 10, 4c.

sources. In the commentaries of St. Thomas upon those Aristotelian passages his actual meaning has to be sought, and laboriously. There the force of the notions "first" and "secondary" in regard to movents is explained in terms of entitative dependence and independence, not in terms of mathematical order. In that setting the notions do not at all preclude the philosophical possibility of an infinite series of moved movents. Rather, they remain entirely compatible with such a series.

People who recognize the validity of this argument of Aquinas today—and they are legion—face serious challenges from modern philosophy. They have to establish the detective rather than cartographic nature of metaphysics. They have to show that human thought commences not from anything interior to its own workings like a Kantian phenomenon, but from sensible things that have being in themselves in an external world. But one thing the upholders of the argument do not have to presuppose is the impossibility of an infinite series of moved movents. What they have to request is that the argument of Aquinas be not placed in the setting of a non-existential metaphysics like that of Aristotle, nor on the abstract plane of modern logic. It has to be examined according to the function of metaphysics in the procedure of St. Thomas himself. For Aquinas "the logician considers the way of predicating, and not the thing's existence". The metaphysician, on the other hand, is "a philosopher who seeks the existence of things", and who therefore seeks causes that are entirely outside the formal notion of the thing, namely the motive and the one who did the deed.<sup>1</sup>

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<sup>1</sup> "Logicus enim considerat modum praedicandi, et non existentiam rei. . . . Sed philosophus qui existentiam quaerit rerum, finem vel agentem, cum sint extrinseca, non comprehendit sub quod quid erat esse" *In VII Metaph.*, lect 17, Cathala-Spiazzi no 1658.

## PROFESSOR AMBROSE ON PROOF

I wish to comment on that part of Professor Ambrose's paper<sup>1</sup> which begins (p. 439), "... it will be enlightening to note certain facts about sentences expressing necessary propositions, i.e. the kind of propositions figuring within mathematics". I want to suggest that her explanations of how necessary propositions figure in mathematics are not all convincing.

Ambrose says at least one odd thing about necessary propositions, and she does not, perhaps, say enough about them. She says, "If the phrase 'heptagon constructed with straight edge and compass' did not have a descriptive use, then it would describe something which would falsify a necessary proposition." But I do not know of any use of "necessary proposition" in connection with which one sensibly speaks of "something which would falsify a necessary proposition", and Ambrose does not explain what she takes a necessary proposition to be. In fact, since she does not explain and since this concept figures so essentially in this part of her paper it is worthwhile pointing out that what it is never emerges from her discussion. It is, except for one characteristic, featureless.

Necessary propositions, in this account, are distinguishable from other kinds, but their only distinguishing feature seems to be that they signify that the words in the sentence expressing them have a use if this sentence occurs as the last line of a proof. Consider three statements on page 440

(1) To know the truth-value of what is expressed by a sentence terminating the statement of proof is to know *what* is expressed.

One could also say, quite evidently without any loss to any mathematician -

(1') To see the colour of the sentence terminating the statement of proof is to know *what* is expressed.

(2) What proposition this combination of words asserts we do not know, nor therefore whether "odd perfect number" has a use

We may as well say

(2') What colour this combination of words has we do not know, nor therefore whether "odd perfect number" has a use.

(3) Prior to finding out whether its descriptive part [There are no odd perfect numbers] has a use he does not know what the proposition is which is expressed by it, whether an impossible or necessary one

Or,

(3') Prior to finding out whether its descriptive part has a use he does not know whether the sentence is red or green.

<sup>1</sup> Alice Ambrose, "Proof and the Theorem Proved", *MIND*, October 1959, p. 435.

Ambrose then declares that (3) and (1) are good reasons for saying that one understands after proof something different from what one understands beforehand. This is evidently true if all that is meant is that one understands *that* the statement is provable, but she, I think, means to say more. One also understands after reading a defective proof something different than one understood beforehand.

A proposition gains *acceptance* after it is proved, and one may *regard* it differently, but Ambrose has not shown how, or that it acquires *sense*, or even a different sense, after proof. The sense of a mathematical proposition is not the same as its acceptance. One may object that although one *learns* from the fact that the proof exists, that the proposition is a theorem, this is something one now knows *about* the proposition, just as one knows something *about* a statement when he knows it to be true. Ambrose's statement that "... only after proof do we understand a sentence in effect merely restricts the scope of the word 'understand' " seems to just beg the question.

Wittgenstein said, " ' Understanding a mathematical proposition ' —that is a very vague concept<sup>1</sup> ". But granting this vagueness we do not say that a man who can correctly define the words of a theorem, is familiar with its grammatical structure, and has been assured of its truth, by divine revelation or some other means, necessarily understands the theorem. It would seem that his understanding of a theorem is guaranteed only by his sure understanding of the *body* of the proof. This is certainly more pertinent to understanding a proof than the simple knowledge that there is a proof I am sure, further, that any account of how a mathematical statement acquires sense must be stated in terms of the naturalness of the tie between the statement and the rest of the mathematical system. This naturalness guarantees acceptance of the theorem. The outer boundary of naturalness is, of course, set by the heat of mathematicians' disputes

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<sup>1</sup> Ludwig Wittgenstein, *Remarks on the Foundations of Mathematics*, p 155.

## MRS. NICHOLSON ON SCIENCE AND MORALITY

(MIND, LIX, April, 1960, pp 259-262)

Mrs. NICHOLSON objects to my view that "a conceptual frame-work deserves to be called cognitive if it helps us to discover or to become aware of some important or interesting features of experience" on the grounds that the epithets "important" and "interesting" are of primarily subjective significance. This is not necessarily the case. When a scientific academy issues a statement that the scientist X has made an important or interesting discovery the words "important" or "interesting" certainly do not have a mere subjective significance. For the academy is apparently in possession of evidence why this discovery is presumed to be important or of interest. This evidence is likely to be in harmony with all the objective standards of scientific achievement currently accepted in the world of science. Hence to say that every time the words "important" or "interesting" are used one is entitled to regard the proposition containing them as only subjectively significant is a mistake. (As another example consider an official pronouncement of the type. "The Parliament considers this issue of utmost importance for the safety of the country.") The use of these words indicates that the claims made *can* be fully justified and verified in the light of the presently available knowledge in that particular area. It would be wasteful and pedantic always to include all of the evidence and justification which is actually at our disposal.

Mrs Nicholson rightly insists that a choice of a conceptual frame-work in terms of which phenomena—physical events and human actions—are explained and appraised is not a matter of taste. It is a matter of fact whether a given framework is successful in accomplishing certain desired ends. Thus if the farmer's aim is to predict reliably future events, we may *find out* which framework (scientific, astrological, magical) helps him better to achieve that aim. Nevertheless, the choice of the framework is a hypothetical imperative, because it depends on the prior desire to be successful in predicting and controlling certain events. Mrs Nicholson recognizes this in saying that "it will be more rational for him, *if he wishes to be a successful farmer*, to adopt a scientific framework" (*italics mine*).

A part of being rational in this context is to commit oneself to scientific farming, to recognize that the scientific attitude is preferable to any other. And this is a matter of choice, of recognizing that rationality prescribes the adoption of the scientific framework. Thus it is not a matter of taste whether to adopt the scientific framework, *if* one is already committed to the aim of understanding and controlling natural events. But this latter commitment must be presupposed and is the result of choice, rational choice, no doubt.

I agree with Mrs Nicholson that it would be inconsistent to accept conflicting frameworks. But she does not show that the moral and

scientific frameworks conflict. She only brings up the suggestion that the problem of freedom might cause difficulties and show that the principles of the two frameworks conflict: "One of the problems confronting the moral philosopher is that of reconciling the moral prerequisite of freedom with the deterministic framework of many scientists and philosophers." One of the aims of my "Science and Morality" was to show that this argument involves a fundamental confusion. The question of freedom cannot be a source of conflict because in adopting a scientific framework one also makes a free decision. What does it mean to say that a scientist has a deterministic framework? It means that he is looking for causal connections among the phenomena he is investigating. But does he understand his own activity as a scientist in purely causal terms? I do not think he does. If he did he would not be a scientist, i.e. a man who believes that he can rationally discriminate between good and bad scientific procedures, between fruitful and unfruitful hypotheses and theories. Part of his activity as a scientist is to criticize scientific explanations which do not seem to come up to the desirable standards of verification, confirmation, etc.

It is true that scientists "promise to offer causal explanations of our motives and actions, as well as of physical phenomena". But do they also promise to offer causal explanations of scientific explanations? If they were satisfied with mere causal accounts of scientific explanations they would be utterly misunderstanding their own activity, which always involves the use of some presupposed rational standards. What I am suggesting is that the problem of freedom lies, so to speak, in back of both science and morality, and cannot be a wedge separating the two areas of rational discrimination. In offering a scientific explanation a man manifests some degree of freedom from antecedent determining causes, and so does a man who offers a moral justification. Both presuppose and appeal to some appropriate norms which may or may not be acceptable to the persons addressed.

I suggest that the pot (science) cannot call the kettle (morality) black, because both are "tainted" by the "problem of freedom". Furthermore, I suspect that the principles of science could not be shown to conflict with principles of morality (unlike two different scientific or moral frameworks) in virtue of the fact that their domains are different. Science deals with what *is*, while morality deals with what *is to be done*. We often argue at cross purposes because we confuse the two domains. But there is no reason why we should not try to be rational in both of them at the same time.

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## G E. MOORE

IN his article on Moore's ethical views in the October number of *MIND*, 1961, Broad refers to Moore's statement in the Schilpp volume that he was strongly inclined to hold that, when a person says "X is right," he is not asserting anything that could be true or false, and to Moore's further admission that he could not say whether his own inclination to hold this view was stronger or weaker than his inclination to retain his old view.<sup>1</sup> I think I ought to mention that Moore completely retracted this statement in the later years of his life (and here Blanshard would confirm what I say). Moore told me orally that he still held to his old view, and further that he could not imagine whatever in the world had induced him to say that he was almost equally inclined to hold the other view. How far he still was from Stevenson was brought out still more strikingly in the course of the conversation when he said he thought that true judgments of intrinsic value were all "logically necessary". (They would of course have to be synthetic *a priori* on his view) I do not remember precisely when this conversation took place, but I have no doubt it was in the last five years or so of his life

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## WITTGENSTEIN AND THE VIENNA CIRCLE

I WRITE in order to correct a factual mistake in Mr K W Rankin's otherwise excellent review in *MIND*, vol lxxi, No 281, of 'The Philosophy of C D Broad' in what I may call 'Schilpp's Library of Moribund Philosophers'

At the end of the first paragraph of page 123 Mr Rankin speaks of 'the factually inept quip (pp 811-812) which moves him' (C D B) 'as late as 1955 to identify Wittgenstein with the Vienna Circle'. In the next sentence he adds 'A legitimate lack of sympathy with his younger colleagues does not mitigate this degree of misrepresentation'.

May I say that this is a complete mare's nest? I very much doubt whether the 'quip' in question (which is explicitly concerned with *phenomenalists*, and refers only obliquely to Wittgenstein) can reasonably be made to bear the inference which Mr Rankin draws from it. But, however that may be, I was well aware that Wittgenstein would have indignantly repudiated membership of, or affiliation with, the Vienna Circle, and I had not the faintest intention of suggesting anything to the contrary

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<sup>1</sup> *MIND*, vol. 70, p. 443.



## VII.—CRITICAL NOTICES

*Modes of Referring and the Problem of Universals.* By D. S. SHWAYDER. University of California Press, 1961. Pp x + 164.

THE first two chapters of Mr. Shwayder's monograph are devoted to a general discussion of reference. In the third chapter he seeks to elucidate the notion of a property—in his own rather specialised sense of this word—by discussing the nature and conditions of reference to properties. In the fourth and last chapter he applies the same method to the elucidation of the notion of a natural number.

Mr. Shwayder loads every rift of his subject with classifications, distinctions, technical terms and their definitions. It is not always easy to follow the course of the exposition, carried as it is by a special terminology not always clearly explained. But the book, though baffling at some points, is continuously interesting and in parts excellent.

Minor difficulties with Mr. Shwayder's definitions begin at the very outset. He introduces the phrase "ostensible referring expression", abbreviated as "o.r.e." (p. 7), and appears to oscillate between two definitions of it. According to one definition an expression is an o.r.e. if it is "customarily used" with a certain "ostensible purpose" (*viz* the purpose of referring to an object) or if the "rules of language" make it "available" for use with this purpose. According to the other, an expression is an o.r.e. only on those occasions on which it is actually being used with this ostensible purpose. An expression will be an o.r.e. in the first sense if it is "cut out" to be an o.r.e. in the second. The definition of "referring expression" ("r.e.") does not, I think, suffer from this kind of ambiguity. To be an r.e., an expression must be *successfully* used to refer to some object. This does not, I think, mean: once an r.e., always an r.e. One and the same expression, I take it, could, without change of sense, be an r.e. on some occasions and not on others.

To understand these definitions, we must know what counts as *success* in referring. Two conditions are laid down by Mr. Shwayder, in terms of *existence* and *identification*. It must be the case that there is something being referred to and it must be the case that the expression used *identifies* the thing being referred to. It is far from clear what Mr. Shwayder means by 'identify'. A preliminary confusion might arise from the fact that in a footnote on page 5 Mr. Shwayder writes "The word 'a' as in 'a (certain specific but I cannot or do not care to specify which) man called this afternoon' provides another rather interesting device for forming r.e.'s". *Success*, with this use of 'a man', clearly does not require that the expression identifies the object referred to as the particular man he is. So it seems that we must take it that the requirement of identification is here satisfied by the specification of the *kind* of object to

which the particular object spoken of belongs. But then it is absolutely clear from the body of Mr. Shwayder's work that he normally understands success in referring to require a sort of identification which the illustrated use of 'a man' implicitly disclaims the ability or will to provide. So it looks as if Mr. Shwayder will have *either* (a) to give up counting such expressions as 'a man' as o.r.e.s or (b) to distinguish two kinds of r.e.s and say either (i) that his identifying requirement holds only for one of these kinds or (ii) that there are two kinds of identifying requirement, one for each kind of r.e. To take either course under (b) will raise further questions about when and why something qualifies as an o.r.e. But in view of Mr. Shwayder's general preoccupations, it seems best to ignore these questions and the footnote which prompts them, and to concentrate on the more stringent requirement for identification.

Obscurities enough surround this notion. One might naturally suppose that the notion of identifying was essentially relative to the audience of a given speech-act, that when one's o.r.e. is indeed an r.e., one succeeds in identifying the thing referred to *for an audience*. A speaker (*S*) might be said to achieve this success if *A* (the audience), hearing *S*'s o.r.e., thereupon knows which or what particular object is being spoken of by *S*. *A* might be said to know which or what particular object is being spoken of by *S* if and only if there is an object that such (i) *A* has identifying knowledge of that object and (ii) *A* further knows that that object is the object being spoken of by *S*; and *A* has identifying knowledge of an object in the sense required at (i) if and only if there is a way or ways in which *A* is able to distinguish that object from all others (e.g. as falling uniquely under a certain description) such that the feature or features by which he is able to distinguish the object do not essentially include the feature of being currently spoken of by *S*. Here there are two broad possibilities (capable of further refinement and subdivision) which it is important to distinguish. *A* may independently possess identifying knowledge of the object or he may glean such identifying knowledge from the speaker's choice of o.r.e., and of these two possibilities the first, I should maintain, is fundamental to the topic of identifying reference.

Does Mr. Shwayder understand his identifying requirement in the sense of this notion of audience-relative identification which I have all too roughly sketched? It is hard to say. He says, indeed. "We identify an object when we refer to it in such a way as to leave no question what we are referring to" (p. 15). But we might leave a question in some minds and not in others. There is no explicit allusion to an audience, no hint that where an audience has more than one member, an o.r.e. might be an r.e. for some, and not for other, members of the audience. And if Mr. Shwayder does, understand his identifying requirement in the way I have sketched, then it is a puzzling fact that he chooses the terminology he does choose to mark a certain distinction. He distinguishes between

those references in making which we exploit the presence of the object referred to (employing, perhaps, the demonstrative 'this'), and those in which we do not; and he names the former "non-identifying references" (p. 18, n. 12). Again, on page 22, footnote 16, he says: "A reference to an object in a location does not expressly identify the referent, but exploits its presence in the situation." In spite of this choice of terminology, he admits that such a reference "does, in a certain sense, identify" (p. 18). But why only "in a certain sense"? If my rough sketch of the identifying requirement answers to Mr. Shwayder's intentions, then such references, when successful, identify *tout court*. If not, then what is the central sense in which it is required of a successful one that it identify?

We might turn, for further light on this question, to what Mr. Shwayder says about "locations" and "logically grounded individuating features". Mr. Shwayder distinguishes, quite correctly to my mind, between identifying in the sense in which one who makes a successful reference identifies the object to which he refers, and identifying in the sense of making a statement of identity. He notes, correctly, that successful identification in the first sense does not necessarily rule out the need for identification in the second. (E.g. the fact that *A* knows who *S* is referring to by the expression 'The man who opened the discussion yesterday' does not preclude *A*'s asking who that man was, in the hope of being answered with an identity-statement.) Now Mr. Shwayder says that we can "fix" the first sense of 'identify' by reference to the notion of the "location" of an object (p. 16). A location, or ostensible location, of an object is explained as something that is "determined" by a "logically grounded individuating feature" (pp. 15-16, 31), and a logically grounded individuating feature is something that is specified by an expression such that we know "in point of logic" that the expression applies, or refers, to at most one object. So such an expression will be "location-determining". There is more than a hint in Mr. Shwayder's writing to the effect that the fact that a given expression is location-determining, or a given feature a logically grounded individuating feature, is in some particularly intimate way connected with the "metaphysical category" of the object, if any, to which it applies, or—in other words of his—with the identity of the "department of language" to which talk of such objects belongs (cf. 16, 27, 30). But he does not elucidate this view and his examples scarcely support it. Thus we might indeed say of such expressions as "first prime number between 20 and 30" (p. 21) and "smallest number, *t*, for which  $2^t + 1$  is not prime" (p. 20) that we know "in point of logic" that each applies to at most one object. But the point of logic in question seems to relate, at least in part, to the expression 'first' and to the superlative termination '-est', taken together with the grammatically singular number of the following noun. These indeed do belong to a class of expressions or

constructions (which includes also the word 'only', similarly followed) which make explicit claims to uniqueness in a certain respect, but they appear to be available for this purpose in connection with any "metaphysical category" whatever.

We must consider a little further what the role of these explicit uniqueness-claimers is in Mr Shwayder's thinking at this point. Another form of example he gives is the following. "we may... uniquely locate a material object by establishing that at some particular time it *completely*<sup>1</sup> occupied a certain region of space" (p. 16) This seems a very remarkable way of uniquely locating a material object. (How would the region of space *completely* occupied at a certain time be, say, a certain chair be itself specified?) And we might think that Mr Shwayder's purposes would be equally well served by a description of the material object in question as the *only* material object of a certain kind in a certain region at a certain time. If so, "logically grounded individuating feature" means no more *in general* than "individuating feature", i.e. feature which can be truly ascribed to a certain object and to no other. The word 'only' simply serves to claim explicitly, with regard to a feature, that it is individuating. We must note, however, with regard to certain features, that we cannot readily separate a bare uniqueness-claim from the specification of the feature in their case simply by detaching a single word or phrase (e.g. 'only'). We can do this in the case of 'only man who has reached the top of Mt. Everest', we cannot in the case of 'first man to reach the top of Mt. Everest'. Perhaps Mr. Shwayder intends only these features for which this separation cannot be so effected to count as location-determining features. Or perhaps, again, Mr Shwayder would not count *either* of these expressions as location-determining expressions in view of the position occupied in each by the proper name 'Mt. Everest' we could scarcely say of either that we know "in point of logic" alone that it has at most one application.

In whatever way we construe "location-determining feature", we must inquire how Mr. Shwayder intends to use this concept to "fix" the still problematic sense of 'identify'. He in no way makes the intended connection explicit. He can scarcely mean that an r.e. has explicitly to *specify* a location-determining feature. Proper names are admitted as central cases of o.r.e.s and, hence, when successfully used, as central cases of r.e.s. Perhaps, in terms of the notion of identifying for an audience, we might interpret the connection as follows. When a reference succeeds in identifying, it does so *either* by *supplying* the audience with knowledge of a location-determining feature of the object in question, at the same time making it clear that it is that object which is being referred to, or by *invoking* such knowledge already in possession of the audience, at the same time making it clear that it is the object of which the audience has this knowledge which is being referred to, or by *directing* the

<sup>1</sup> My italics

attention of the audience in such a way that it acquires such knowledge, etc. In brief, a reference succeeds if and only if the audience has, or then acquires, knowledge of an individuating feature of the object being referred to and knows that it is the object of which it has this knowledge which is being referred to. Then, I suppose, we can widen or narrow the range of genuine identification in Mr. Shwayder's central sense by interpreting more or less widely the notions of 'having knowledge' and 'individuating feature'. Thus a man who responds appropriately to the demonstrative reference 'That dog' might be said, in a sense, to know that there is just one dog in his field of vision and hence to know an individuating feature of the object referred to, but not to have reflected upon or formulated to himself this proposition, and hence not to have knowledge of it in a more "theoretical" sense (*cf.* p. 18). But how reflective does an audience have to be? If a speaker refers to my friend by name and I respond appropriately, do I—in order for the identification to count as identification in some central sense—have, at the moment of reference, to reflect upon such a feature as *being both known to me and bearing that name* (which may in fact be an individuating feature) or—worse—upon such a feature as *being the first man I met with gold in his teeth* (which must be an individuating feature if anything has it)? It would seem gratuitous to insist on such high standards of audience-reflectiveness and still more gratuitous to insist that the feature reflected on has to be such that it must be individuating if anything has it. But would there be any non-arbitrary way of relaxing the standards so as to allow proper names a central place in successful identification while keeping demonstrative reference on the periphery?

I must emphasise that this attempt at interpreting Mr. Shwayder's intentions in terms of the notion of successful identification for an audience is only a guess. Mr. Shwayder does not mention audiences. Yet it must surely be a guess in the right area. For what else could 'success' mean? There are other, *related* things it might mean. It might, for example, mean using an expression which (a) is "cut out" by the "rules of language" for referring use (*i.e.* for bringing about identification in the sense I have suggested) and (b) is *correctly* used in application to the object it is applied to on a given occasion. Whether it actually succeeds with the audience on that occasion would be irrelevant to its success in *this* sense, even though this sense is explained in terms of the other. Perhaps this is nearer to Mr. Shwayder's intentions. But since this sense is explained in terms of the other, it is hard to see in what ways different from those I have suggested we could use the notions of location-determination and individuating features to "fix" this sense of identification. Sometimes one is tempted to suppose that Mr. Shwayder really thinks of the paradigm case of success in reference as the use of an expression such that its meaning alone guarantees uniqueness of application for it, he is looking ahead, after all, to his terminal problems of

reference to universals. But though this thought may influence him, it does not seem to be his official view.

Mr. Shwayder's first chapter, to which the above remarks relate, is rich in doctrines calling for scarcely less discussion. I will briefly mention one or two more. (1) As already remarked, Mr. Shwayder points out that a successful identification in the sense we have been concerned with does not necessarily render superfluous identifications of another kind, *viz.* those that are supplied by *statements* of identity. We can perfectly well have identifying knowledge of an object in respect of two different "locations"—knowledge invoked or supplied by two different *o.r.e.s*—without knowing it is the same object in each case. Thus *en passant* solution of Mr. Shwayder's of the spurious old problem of how statements of identity can be informative seems to me quite correct. (2) Mr. Shwayder suggests that, for certain subject-matters, there are preferred identifying expressions such that one for whom they successfully identify (in the first sense) the object cannot then be in need of further identifications (in the second sense) of that object. Mr. Shwayder says of such expressions that they specify a "primary location" of the object. An example he gives is the numeral '5', used to refer to the number 5. One for whom the reference was successful *could* not then need to ask 'Which number is the number 5?' in just the way in which he might, for example, need to ask 'Which number is the cube of the square of 27?' (If I have understood Mr. Shwayder correctly here, however, he is in some confusion about his own point. He says the ruled-out question is: 'Which number 5 do you mean?' (p. 21). But this form of question is ruled out for *any* successful reference and not just for one which specifies a "primary location".) Mr. Shwayder then suggests that where primary locations are available, the logically fundamental method of showing that an object specified in respect of one non-primary location is identical with an object specified in respect of another is to show that both locations are connected, in the way of object-identity with the primary location. Mr. Shwayder is inclined, on page 22, to suppose that "temporal" objects do not have primary locations, though in a subsequently added footnote on the same page he toys with the suggestion that primary-location references in abstract subject-matters somehow correspond to ("take the place of") demonstrative references to actually present material objects, and on page 59 he says of personal names that "having such and such a proper name stands in for primary location", adding, in a further footnote on page 60, that this is "at best only an approximation". Here, as quite often, one feels that Mr. Shwayder finds the scent of logical analogy equally difficult either to resist or to pursue—to its possibly deceptive source.

Mr. Shwayder's second chapter is divided into two parts. In the first he lists a number of characteristics which *o.r.e.s* may have or lack, and in the second he discusses, in the light of this list, what he describes as a "haphazard" selection of varieties of *o.r.e*. The

governing principle of the listing of characteristics—a principle not very clearly or felicitously explained in the opening pages—seems to be as follows. It may be that the use of an expression to refer to a particular object is rendered correct by nothing but a purely arbitrary convention licensing the application of the expression to that particular object. That is, this application of the expression does not form part of any system and is not governed or guided by any general rule of language. In such a case we have a totally arbitrary o.r.e. (e.g. some proper names, and, Mr. Shwayder also suggests, the Greek mathematicians' use of  $\pi$ ). There are different, not mutually exclusive ways in which an o.r.e. may be partially or totally non-arbitrary. Thus (a) there may be a *systematic* element in the assignment of o.r.e.s (e.g. the numbers of houses in a street, surnames, the use of quotation in referring to expressions). The application of o.r.e.s may be partially or wholly governed by (b) general *indicative* conventions ('I', 'this') or (c) general *descriptive* conventions ('the old man'). Some designations, perhaps better classified as proper names than as descriptions, may nevertheless be (d) *appropriate* ('Goldbach's Conjecture', 'The French Defence'). Finally—a classification which becomes important later in the book—some o.r.e.s may be (e) *secondary*, their use presupposing another use of the same or a related expression (e.g. the use of numerals to refer to numbers, the uses of 'blue' or 'green' as colour-referring nouns).

The varieties of o.r.e. which Mr. Shwayder then proceeds to discuss include, among others, proper names, pronouns, definite descriptions, quoted expressions and numerals. Mr. Shwayder's account of what definite descriptions are—*viz.* "expressions formulated in or translated into English as 'the' followed by an adjectival word or phrase followed by a common noun"—seems to be oddly restrictive and to exclude some celebrated examples. But he is good on proper names; and, though there is some obscurity, there is also shrewd observation in this relatively uncontroversial chapter.

Mr. Shwayder's third chapter is suggestive, but difficult and arbitrary. It is packed with ideas, but seems over-hastily packed. He begins by turning his attention from referring uses to "characterising" or "distinguishing" uses. With certain exceptions, any "common term" which is capable of being applied, in the same sense, to a number of different objects, is capable of distinguishing or characterising use; and has distinguishing or characterising use when it actually is applied, in the way of predication, to an object identifiably referred to by another expression. (Distinguishing use, like referring use, however, is not confined to predications, *i.e.* to statements, but may occur in other kinds of speech-act as well.) Various respects in which distinguishing uses, or terms having distinguishing uses, may be classified are then listed and commented on. Thus such a term may be one-, two- or *n*-place; it will have some degree of *contrariness*, *i.e.* a more or less limited or an unlimited

range of contraries, it will have some *degree of freedom*, i.e. will be combinable in application to one and the same object with a more or less limited or an unlimited number of terms all belonging to different contrariety-ranges, its use as a distinguishing term will either be its *primitive* use or will be *derivable* from some non-predicative use; either the test for its applicability will involve determining the presence of some other definite characterising feature (in which case it is *subordinate*) or it will not involve this (in which case it is *basic*); it will be either *identificatory*, as are, "regularly", nouns in distinguishing use preceded by the indefinite article, or *descriptive*, as are, "regularly", adjectives in distinguishing use. There are other classifications in Mr Shwayder's list, which I shall not mention. Excluded by him from distinguishing use are "verbs and participles in simple temporal statements" of which he gives as examples, 'He was running' and 'He caught the rabbit'.

Mr. Shwayder's comments on these classifications give rise to many more difficulties and doubts than I shall mention. His way of "setting off" simple temporal statements from distinguishing statements, for example, is utterly mystifying. He says that a temporal statement is a statement for which the fundamental verificatory test (a) can be applied only on "one, or at most a small number, of discrete occasions" and (b) "involves no operations upon the object" (such as "comparing it to a standard or measuring it") but "only a passive observation of it" (p. 89). We might say of a blushing boy's face that it was red, or of a sanguine-complexioned boy's face that it was red. Do we "compare with a standard" in the one case and not in the other? Or is the first statement not a temporal statement? Then is 'He is blushing' not a temporal statement? Is 'Napoleon conquered Europe' a temporal statement or not? More thought is needed about this distinction. Again, Mr Shwayder says that dispositional qualities are subordinate (p. 80), but since the tests for many dispositional qualities presumably turn essentially on verifying simple temporal statements, and such statements do not, in Mr. Shwayder's view, ascribe distinguishing features to things, it seems to follow that such dispositional qualities do *not* qualify as subordinate in Mr. Shwayder's sense. Nor, clearly, do they qualify as basic (cf. p. 80). Yet "every distinguishing use is either basic or subordinate" (p. 80). Again and again Mr. Shwayder rests his distinctions on a couple of examples and a sketchy and highly abstract characterization which are quite inadequate to make his intentions clear even where they do not leave the impression that he simply has no clear intentions.

Mr Shwayder next develops his doctrine of *o.r.e.s* which refer to properties. His central points seem to be these (1) Property-referring uses presuppose distinguishing uses belonging to the same family. (The property-referring uses are "secondary", the distinguishing uses "primary".) This does not mean that for every property-referring use which we make of a term, there must be



antedecedently in existence an exactly corresponding distinguishing use of that, or a related, term. Our mathematical theory, for example, might licence the introduction of certain shape-*ores* without our ever having had occasion to introduce or use a corresponding term for characterising particular spatial objects. But *some* shape-distinguishing uses there must be, before any shape-referring uses can exist. Further, with every novelty in property-reference, we add to the possibilities of object-characterisation. Property-referring uses descend from distinguishing uses, but in turn beget further distinguishing uses. (2) There can be terms used for reference to universals other than *properties*. Indeed, under the single heading of 'colours', we can distinguish different kinds of universals which may be objects of reference. But we get reference to colours as *properties* only when we have discovered and introduced criteria for distinguishing and identifying an unlimited number of colours; as, in our era, scientists have come to distinguish colours by wave-length, saturation and intensity. And so for all reference to *properties* Mr. Shwayder acknowledges that this is an arbitrary stipulation on his part regarding the use of the word 'property'. (3) The object of reference of a property-referring use is most commonly said by Mr. Shwayder to be "nothing other than the associated distinguishing use". This doctrine is not confined to reference to *properties*, in Mr. Shwayder's restricted sense of this word, but is extended by him to all reference to universals. Sometimes Mr. Shwayder says that this doctrine is not quite accurate: an improvement which would, however, make but a "trifling" difference (chap. II, p. 48), would be to say that the object of reference is not the distinguishing use, but "that element which the realisation of the primary [*i.e.* here, the distinguishing] use would contribute to the success of language acts"; and this element contributed to the success of language acts is sometimes said to be, *e.g.* not a colour-distinguishing use, but a *distinction* of colour. Sometimes, again, Mr. Shwayder identifies the referent in what might seem to be yet a third way, but is more probably merely a restatement of the first: the referent of the universal-referring use is the *possibility* of characterising objects with the characterising use.

All, or most, forms of this last doctrine of Mr. Shwayder's seem exposed to obvious objections. If I say that green is my favourite colour, I do not seem to be saying that a certain use of language is my favourite colour, nor that the possibility of this use is, nor that the contribution made by this use to the success of a language-act is. Possibly Mr. Shwayder's "trifling" amendment, properly understood, takes care of this crude objection; for he says that the "element contributed" etc. is a distinction of colour; which seems near to saying that it is a colour. But it seems difficult to understand the amendment in this way, and difficult to see what's left of the original doctrine if it is so understood.

I think Mr. Shwayder errs here in trying to force a general form of

answer to the question 'What is referred to in such-and-such a secondary referring use?' out of his theory of secondary and primary uses. Non-bizarre answers to such questions would always, I think, be internal to the secondary uses of language in question. But I do not think the error is very grave. The theory of primary and secondary uses is important, and its importance survives the retracing of the reductive step which Mr. Shwayder takes.

It is taken again in Mr. Shwayder's interesting and valuable last chapter on natural numbers. Numerals, as employed to refer to natural numbers, have a secondary referring use, what they refer to is the primary use they have, *viz.* their use in *counting*. The counting use of numerals is neither a referring nor a distinguishing use. It is intimately associated with, though distinguishable from, other uses which numerals may have. *e.g.* the use of the terminal numeral in a counting operation to answer the question 'How many?' (the *cardinal* use), the use of numerals as systematic *o.r.e.s* to refer to numbered objects (*e.g.* houses in a street); the *ordinal* use of numerals (a distinguishing use), the use of numerals as "calculating markers" in arithmetical calculation (the *calculating* use), and their use as secondary *o.r.e.s* referring to numbers in true statements about numbers (*e.g.*  $7 + 5 = 12$ ). Mr. Shwayder's list of uses is longer than this, but this will serve for present purposes.

There is something a little misleading about the way in which Mr. Shwayder presents his list of uses. He is right to stress that when we count a collection of objects, the use which we make of the numerals in counting is different from the use which we make of the terminal numeral in answering the question 'How many?', and when we count with the object of arriving at an ordinal use, the counting use is again different from the ordinal use. But it is not as if there were a *primary* operation of counting such that no distinguishable terminal use at all was envisaged as the termination of an act of this primary kind. I do not think Mr. Shwayder would disagree with this. "Knowing how to use the system (of counting-numerals), we may say of any counting numeral that its use is to terminate counting at a certain point" (p. 146). But not just when we feel we have done enough of *counting*, and would like to embark on some other activity.

Mr. Shwayder's problem is to show how the use of numerals to refer to numbers (the use of "number-numerals") is derived from and dependent upon the counting, or, as I should prefer to say, upon the counting-cum-cardinal, use. He points out that the counting use essentially involves the introduction and employment of an ordered sequence of distinguishable and identifiable numerals; he shows admirably how a limited practical mastery of the counting-cum-cardinal use may naturally lead on to a practical grasp of a technique for supplying oneself with indefinitely many numerals, how, given these two achievements, the provision of basic criteria for distinguishing and identifying an unlimited number of numbers

does not, as in the case of reference to properties, represent a problem to be solved, but one whose solution is already given. What of statements about numbers, the statements, at least, which belong to the arithmetic of natural numbers? They embody, Mr. Shwayder says, the theory of counting uses or, better, the theory of the "composition" of countings. (We "compose" countings if, e.g., we count two collections separately and then count all their members together.) We can look at arithmetic in three connected ways. First, we may see it as a pure calculus, a system of rules for generating formulas out of formulas ("calculating"), independently of any interpretation being assigned to the signs of the system. Second, we may interpret (some) arithmetical formulas as "special forms of usually but not invariably true contingent statements about counting operations" (p. 152). Third, we may see these statements as setting a *standard* of what arithmetical compoundability is, and "there-with" the relevant equations assume the status of statements about numbers (p. 153). So (some) statements about numbers are effectively standard-setting statements about the counting uses of numerals.

Has Mr. Shwayder satisfactorily answered his own question about "how these equations come to express genuine statements of identity about numbers"? (p. 154). His emphasis on the contingent character of his "statements about counting operations" leads one to suppose that he is thinking primarily of the utility of applying arithmetical formulas and arithmetical calculation to concrete countables. This is no doubt as important a factor as you please in the genesis of the arithmetical calculus. But must we therefore say that arithmetical identities acquire the status of statements about numbers essentially by serving as a standard for the arithmetical compoundability of countings of concrete countables? The systematically ordered numerals may themselves be counted, as Mr. Shwayder frequently emphasises, and it does not seem that interpretation of the arithmetical equations as statements about certain counting operations conducted on *these* countables would yield contingent statements. So regarded, they would set a standard for the *correctness* with which the operations were conducted rather than for their arithmetical compoundability. *That* is guaranteed. But one does not, in making this point, have to give up Mr. Shwayder's insistence upon the primacy of the counting use of numerals.

Mr. Shwayder is very well aware that he has not given a complete account of the notion of a number. To the more limited task which he sets himself in this chapter he seems to me to bring both imagination and insight.

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*A Hundred Years of Philosophy.* By JOHN PASSMORE. Duckworth, 1957. Pp 523. 35s

IN this admirable book Professor Passmore has most ingeniously fitted himself into the framework of the series in which it appears.

The last hundred years do not form an obviously natural unit in the history of philosophy. A more attractive point from which to begin a recent history of the subject would be 1903: the year of Russell's *Principles of Mathematics* and of Moore's *Principia Ethica* and *Refutation of Idealism*. Others would be 1874, the year of Green's edition of Hume and Bradley's first important essay, or 1865 when Mill's *Examination of Hamilton* and Stirling's *Secret of Hegel* were published. Passmore copes with the difficulty by taking 1843 and Mill's *System of Logic* as his starting-point and compensates for this backwards extension of the temporal range of his study by giving only a fairly cursory treatment in his first two chapters of the naturalist philosophies prevailing in Britain and Europe in the three decades before the effective beginning of absolute idealism in this country. It is a fortunate contingency that the period involved is precisely that of the main literary activity of Lotze, who is the vital connecting link between the philosophy of Hegel and its British reincarnation.

A second interpretative decision, this time about the topical scope of the book, is reasonable enough. Passmore confines himself to the central core of philosophical disciplines: logic, theory of knowledge and metaphysics. All the same this is an impoverishment; the interaction between the centre and the periphery has been an important part of the story for much, if not all, of its length. For example, although the ethical theories of Russell and the logical positivists are clearly epiphenomena, this cannot be said of Bradley or Moore or recent linguistic philosophers. Furthermore the remarkable comprehensiveness of philosophical interests shown by Passmore's other writings suggests that something well worth having has been lost by this circumscription.

The boldest of his expedients of presentation is the solution he adopts to the problem of national perspective. He has neither restricted himself to philosophical work written in English nor attempted the perhaps impossible task of establishing parity of treatment for the philosophy of different linguistic groups. 'I have', he says in the preface, 'deliberately chosen to be insular, exhibiting, however, that kind of insularity which does not rule out an occasional Continental tour or a slightly more extended stay in the United States. . . . My criterion was: to what extent have the ideas of this writer entered into the public domain of philosophical discussion in England?' This simple and sensible device frees him from the responsibility for deciding the nebulous and speculative issue of a philosopher's intrinsic importance. Foreign philosophers are assessed in terms of their impingement on the British scene: as a result James, Carnap, Peirce and Frege are among the twelve most frequently mentioned philosophers in the book, while Brunschvicg and Nicolai Hartmann have to make do with a short paragraph each.

One other general feature of the book should be mentioned at this point. It is not only a most interesting, judicious and readable narrative, it is also an excellent work of reference. There is a good

'short bibliography' of twenty-four pages at the end but the richest bibliographical nourishment is to be found in the footnotes. A vast mass of well-selected periodical literature is referred to in these notes and is made readily accessible by an index of names and a very well-constructed subject-index, itself equipped with useful cross-references, in which asterisks indicate on which page bibliographies are to be found. The footnotes are entertaining as well as useful. Passmore tells the reader where to look for fictional representations of modern philosophers. to *Robert Elsmere* for T. H. Green and to *Sinister Street* for F. C. S. Schiller. He points out that the composite volume *The New Realism* was appropriate to an age of manifestoes and suggests a comparison with Ezra Pound's *Imagist Anthology*. In this connection he does not disdain a little human interest; he observes that Pitkin, one of the contributors to the volume, went on to make his reputation 'by advising a multitude of readers how to be happy though forty'. There are also unclassifiable aperçus such as this, of Lord Haldane. 'In the *Reign Of Relativity* he urges upon British industry the importance of embarking upon an inquiry into the possibility of harnessing atomic energy'. The level of accuracy in this huge accumulation of particular detail is extraordinarily high. Passmore is by no means one of those commentators on foreign philosophy who cannot manage to get the spelling of unfamiliar names quite right nor is he afflicted by the more common weakness of tacking on misremembered initials to well-known surnames. The only error I can find that might be worth amending is at the top of page 421 where '*Treatise on Induction and Probability*' should read '*The Logical Problem of Induction*'.

Like everything else of Passmore's, *A Hundred Years of Philosophy* is extremely well written. It is expressed in what might be called the Australian style, that fresh, terse, confident, uncircumlocutory manner of utterance common to most of the contributors to the *Australasian Journal of Philosophy*. But in Passmore's hands it is a fairly sophisticated literary instrument, witty without being heartily jocular, colloquial without being self-consciously masculine and down-to-earth, lucid without being oversimplifyingly crude. A massively detailed survey of this kind poses a problem of exposition in the very large amount of reported discourse it is bound to contain. Passmore handles this difficulty with great skill. He does not let quotation, with its tendency to interrupt the sequence of exposition, bulk too large and he adroitly rings the changes on that handful of expressions—*view, theory, doctrine, thesis, contention, belief, opinion* and so forth—that are indispensable to a writer with his task. Furthermore he has the intellectual virtues appropriate to his style. He is courageous in tackling difficult, involved and contentious pieces of thinking and clear-headed and unevasive in the precise and definite accounts he gives of them. A substantial instance of this is his admirable treatment of the development of formal logic in the period under review. There are full and detailed accounts of the

innovations of Boole and Peirce and a remarkably luminous exposition of the main theories of Frege, in the course of which Passmore effectively grasps that well-known nettle 'the concept *horse* is not a concept'. His treatment of Hilbert and Brouwer is helpful and sympathetically informed with none of that all too common reliance on slogans which stereotypes formalism into the view that mathematics is a game with meaningless marks and intuitionism into the theory that the law of excluded middle is false. These tiresome mottoes are put into the setting that is essential to their intelligibility. Another field in which he is prepared to consider inquiries of a fair degree of formal technicality is the philosophy of science. The last half of chapter 18 gives a really model account of recent discussions of inductive logic by Popper, von Mises, Reichenbach, Carnap, von Wright, Kneale and Braithwaite. Passmore manages to combine his usual clarity and simplicity with a concrete explanation of technical conceptions vastly superior to the vague explanatory gestures of a more balletistic type of philosophical historian.

In general he is rather self-effacing as far as his own philosophical convictions are concerned. Expounding a theory he will consider its subsequent fate in the philosophical world but his task, as he conceives it, is to report criticisms that have actually been made and been widely accepted rather than to invent new ones or to authorise those he personally prefers. All the same an impression of personal endorsement is given from time to time. At the conclusion of the main narrative, for example, he describes Waismann as a centre of resistance to the ordinary language philosophy of present-day Oxford and goes on 'He is not alone, at Oxford or elsewhere, in believing that ordinary language philosophy will die of inanition'. There is a suggestion that the fulfilment of this prophecy would not be unwelcome to its reporter. Whatever his particular hostilities may be they are not allowed to deprive anyone of fair treatment. But a sufficiently clear general impression is left by his variations of tone and emphasis to allow his general standpoint to be reconstructed and confirming evidence for the reconstruction is available in the statistics of space-allocation as between different philosophers. These figures put Russell and Wittgenstein well in the lead. There follow, in order, Mill, Bradley, James, Carnap, Moore, Alexander and Stout. After Carnap, Ryle and Popper are the most extensively considered of currently active philosophers. The surprisingly high position accorded to Alexander and Stout can to some extent, perhaps, be explained by their Australian connections. Alexander was born there and Stout retired and died there. But Passmore himself is a pupil of John Anderson, who has developed Alexander's ideas in an interestingly idiosyncratic way, and he shares one very fundamental conviction with Alexander: that philosophy is continuous with science and should not be carried on in autonomous isolation from it, whether in the manner of transcendentalist metaphysicians or in that of the analysts of language.

In general Passmore is much more interested in applied philosophy of the sort that responds to problems arising in the concrete work of the sciences and that reacts upon them than in either of the two main varieties of pure philosophy, that which seeks to explain the universe by demonstrative argument or that which derives from conceptual paradox and perplexity. This is shown in some favourably toned remarks about Russell, who, he says, ' belongs in spirit to that tradition of philosophy which conceives it as " the science of the sciences " '. He sees Russell as working ' in the manner of that scientific tradition which first came into vigorous growth, in modern Europe, in the seventeenth century and in striking contrast to the differentiating habits of that scholasticism against which it forcibly reacted and into which, in philosophy at least, it shows some signs of returning '. He is conspicuously better disposed towards Russell himself, with his wide and fertile extra-philosophical interests, than to the philosophically purer Russellian tradition with its concentration on the sense-datum theory of perception. Its leading members are, indeed, treated under separate heads. Price as a belated, Russellized adherent of the school of Cook Wilson, Ayer as logical positivist with some regrettably English and non-scientific propensities. To do this is to take their earliest writings as too authoritative a clue to the general significance of their work. Certainly Price began as a disciple of Prichard and the doctrinal allegiance of *Language, Truth and Logic* is made abundantly clear. But Price's *Perception and Thinking and Experience* and Ayer's *Foundations of Empirical Knowledge* descend from Russell's *External World and Analysis of Mind* and are connected through it to Mill's *Examination of Hamilton* and so eventually to Hume. In emphasizing those aspects of Russell's philosophy to which he is most sympathetic Passmore has perhaps been led into obscuring those of its features on which Russell himself has most concentrated and which have continued to be influential through the work of others.

The metascientific, as against the Humean and epistemological, side of Russell's philosophy is best represented today by Professor Popper and he would appear to be Passmore's favourite contemporary. Popper's programmatic insistence, in his presidential address to the Aristotelian Society and in the introduction to the *Logic of Scientific Discovery*, that the proper task of philosophy is the same as that of science, to achieve an understanding of the world, is a fair expression of Passmore's own view. The appeal that applied, non-autonomous philosophy has for him explains the unfashionably sympathetic attitude he adopts towards Collingwood. In an engagingly blunt footnote he remarks. ' It is sometimes suggested by Idealist admirers of Collingwood that the brain disease from which he began to suffer in 1933 is reflected in his ultimate heterodoxies. When one contemplates the speculative freedom of these later works one can only wish that his contemporaries could have been similarly afflicted.' It is not, I think, just the speculative freedom that

Passmore admires, it is also the fact that in his writings on history, art and the presuppositions of natural science Collingwood had problems to deal with that were provoked by difficulties arising in serious intellectual work rather than by the perplexities of isolated cerebration.

If Alexander gets a little more attention than he deserves and the sense-datum philosophers rather less, this is as far as any distorting effect of Passmore's own beliefs could reasonably be held to extend. The purest of pure philosophers and the heroes of contemporary philosophical fashion, Moore and Wittgenstein, are discussed most fairly, with care and at considerable length. Of the two treatments that of Moore is the less successful though all honour is done to him. 'Moore', Passmore says, 'has a great deal to offer to those who have felt the fascination of his drastic honesty'—The weakness here lies in Passmore's concentration on the explicit doctrinal content of Moore's writing, on his theory of concepts, his distinction of acts and objects, his protracted wrestlings with the problem of analysis, rather than on his far more influential, if more or less unconscious, innovations in philosophical method. The result is a somewhat old-fashioned picture of Moore who is seen in the way in which he must have appeared to the philosophers of the 1930s as someone primarily interesting for his work on the relations between sense-data and material objects. What is left out in this view of Moore as a remote Edwardian figure is his very large part in determining the character, interests and even the literary style of post-Wittgensteinian analytic philosophy. The publication of Moore's 1910 lectures on *Some Main Problems of Philosophy* showed that he had little to add in the way of positive doctrine in the ensuing forty years; yet his central importance as a methodological example for recent linguistic philosophy is one of the few things not in dispute between such supporters of that style of philosophizing as Warnock and such denouncers of it as Gellner.

Passmore's dealings with Wittgenstein deserved the highest praise not because of any lack of intellectual sympathy that may be conjectured but because of the great intrinsic difficulty of the task. He makes clear how, in the *Tractatus*, the ontology of simple objects follows from the theory that a proposition is a concatenation of names and thus, in its turn, from the fundamental assumption that every proposition has a definite sense which lies in the proposition's relation to the world. He lucidly sets out the distinction between the *Tatsache* and the *Sachverhalt* but he does not go into the question of what the predicative elements, if there are any, of the elementary propositions are the names of. He gives full value to Wittgenstein's 'fundamental thought', that the logical constants do not stand for anything, and traces the difference between the senseless, which in a perfect language *need* not be said, and the nonsensical, which *could* not be said. Finally, to illustrate Wittgenstein's view that the propositions of philosophy if sensible at all are



either propositions of psychology or else state what the symbolism should show, he draws an interesting distinction between induction, conceived as the psychological propensity to look for simpler explanations, and the law of causality, taken to be the assertion of the fact, shown by our symbolism, that there are natural laws. The only comparably good account in a short compass of the main drift of the *Tractatus* is Russell's introduction to the work itself. The more straightforward business of describing the *Philosophical Investigations* is also more effectively and sympathetically carried out. He starts from Wittgenstein's rejection of the pursuit of strictly definable essences and its replacement by the detailed, therapeutic description of the actual use of words. Summing up the new theory of meaning as maintaining that not all words are names and that understanding is not a mental process he follows its application in the construction and use of language-games, the criticism of ostension and metaphysical simplicity, the behavioural interpretation of mental states, the rejection of private languages and the expressive theory of reports of feelings. In his bibliographical note to the *Investigations* he oddly fails to mention the remarkable account of the book, similar to his own in clarity and definiteness, given by Feyerabend in the *Philosophical Review* for 1954.

*A Hundred Years of Philosophy* is itself a work of summarization so there would be no point in giving an extended survey of its contents. But the general lay-out of the chapters deserves a mention since it is in the collocations of different philosophers by which order is brought out of historical chaos and conventional lumpings-together, where they exist, amended that the hardest decisions lying behind the book, and perhaps its chief originality, are to be found. The first two chapters set the mid-nineteenth century, pre-idealist, scene; one of them on Mill and orthodox British empiricism, the other on comparatively unprofessional, scientifically-inspired forms of naturalism. Two chapters follow on absolute idealism: the first on its standard expression in Lotze, Green and Bradley, the second on the ensuing debate about the place of personality in the scheme of things whose chief participants were McTaggart, Bosanquet and Royce. With an illuminating flourish the next chapter brings together a host of more or less voluntaristic philosophers, united by their repudiation of the Cartesian principle that nothing but what is clearly and distinctly perceived should be accepted. Amongst others Schopenhauer, Nietzsche, Lange, Bergson, James and the pragmatists in general figure here. Formal logic is then considered in a chapter recounting the central line of development from Whately to Frege and another which deals with an assortment of heretics and oppositionists, in particular Bradley and Bosanquet, Schiller and Dewey. A chapter on 'the movement towards objectivity' links Brentano, Husserl, Meinong and Stout in common resistance to the traditional idea that the objects of the mind are in some fashion dependent upon it. This movement prepared the way for the early

realism, conceptual and perceptual, of Russell and Moore who are discussed in the following chapter. The theme of realism runs through the next three chapters: on the school of Cook Wilson, on the new realists and Alexander and on the critical realists, especially Santayana, and such later naturalists as Nagel and C. I. Lewis. Croce, Collingwood and Stout are the leading figures in a comprehensive chapter on metaphysicians and another such chapter brings together scientist-philosophers from Mach, Clifford and Pearson to Whitehead. The really recent history of the subject begins with a chapter on the *Tractatus* and Ramsey which is followed by an excellent account of classical logical positivism. Tarski, Quine and Popper bulk large in a chapter on logic, semantics and methodology and the main narrative closes with a description of the later philosophy of Wittgenstein and of the parallel movement at Oxford. The book ends with a succinct and informative postscript on existentialism from Kierkegaard to Sartre.

There is nothing strikingly new about the general form which Passmore has given to his story and it would have been an odd story if there had been, but it contains some just and useful pieces of classification, in particular where it brings together the defenders of personality, the anti-intellectualists, the champions of the object and the post-positivist philosophers of deductive and inductive science. The only reasonably coherent movement of thought that has somehow been dissipated in the process is one that has been referred to already in connection with Passmore's treatment of Price and Ayer. The rather fully articulated body of doctrine put together by Russell and Moore in the first decade of the century, and set out in their respective general books on the problems of philosophy written in the years just before the 1914 war, has been the focus of a persistent and respectable philosophical tradition. Broad, Price, Ewing, Kneale, Laird and Stace are among its leading members. Passmore sees no unity here. Broad is presented as a feature of pre-*Tractatus* Cambridge, Price as an unorthodox, latter-day Cook Wilsonian, Kneale as a specialized philosopher of science, Laird as a minor new realist, while the other two are remitted to footnotes. A chapter on the aftermath of realism, placed somewhere between the chapter on Cook Wilson and the account of the logical positivists, could well have been used to consider them. Russell's *Problems of Philosophy* gives an outline sketch of their common convictions. They believe in the existence of substantial, non-analytic necessary propositions, in the logical independence of mind and matter and in the objectivity of universals and they lean towards the causal theory of perception (though Stace is an uncompromising phenomenalist) and the analogical solution to the problem of other minds. They are liberal unionists to Russell's Gladstone, with the *Tractatus* playing the part of Home Rule.

Passmore's account of the very large number of philosophers that he deals with is always fresh and demonstrates clearly that he has relied on their actual writings in preference to such secondary

material as there is. He does not dig very deep in his interpretations confronted with as long and as multifarious a philosophical career as Russell's; he does not try to search out any basic unifying tendencies in the work as a whole. So his largely chronological survey of its contents tends to give the same impression of desultoriness as, on a first inspection, the work itself does. This interpretative restraint is, I think, deliberate. (His chapter on logical positivism, for example, does not embody the general conclusions about that movement which he arrived at in his excellent articles on it in the *Australasian Journal* in the 1940s.) It is part of his policy of producing as objective and unslanted a history as possible. All the same I should have expected a less non-committal version of Passmore's enterprise to have been still better than it is, even if I were not as confident as I am that any deeper interpretations that he might offer would be very well-grounded ones. But in mastering this massive body of material and in presenting it with such style, articulation and accuracy he has done such a conspicuous service that it would be ungrateful to ask for more.

ANTHONY QUINTON

## VIII.—NEW BOOKS

*The Logic of Social Enquiry* By QUENTIN GIBSON. Routledge & Kegan Paul, London, 1960. (International Library of Sociology and Social Reconstruction) Pp. x + 214 24s .

THIS is a work of religious apologetic, the religion being "Scientism". As with other such works its arguments may seem very convincing to those who share the author's faith but make no impression upon those who do not. For Mr Gibson "social enquiry" is and can only be "Science", and this is not just a fact to be noticed, but a dogma to be passionately defended. Half the book is an attack upon "Anti-Scientific Views about Social Enquiry", but no statement of these "anti-scientific views" is produced from anyone who actually holds them and, like a Bible Christian who quotes texts even in controversy with atheists, Mr. Gibson seems not to understand what his opponents maintain and why. The target would appear to be Continental European, and particularly German, sociology, but the fact that his index contains many names of authors who have written in English but very few others, and those all available in English translations, arouses the suspicion that Mr Gibson (who is an Australian) can read no language except his own, and simply does not know what those whom he is criticizing have said. They think teleologically, and of this he seems quite incapable. The question "What is the point of it?" is one which he not only never asks but cannot understand anyone asking. Therefore he naturally never asks what is the point of Science, which he treats as an end in itself, self-evidently justified. But most of us, I think, would say that the purpose of what is nowadays most often meant by "Science" was to predict and, in favourable circumstances control events, and that a scientific method was justified only by its success in this. Mr Gibson can understand that such methods have been thought inapplicable to social studies, but does not try to justify them by quoting any successes they have had, it is enough for him, as for so many religious apologists, to argue that what he accepts on faith has not been demonstratively proved impossible. A thing which he cannot understand at all is that it could seem to anyone that the most that can be achieved on these lines is trivial (the labouring mountains of "social science" bring forth only ridiculous mice), but that other kinds of "social enquiry" may be really valuable. He ascribes to those who hold "anti-scientific views" the claim to have an "alternative procedure" to "scientific procedures", but it does not occur to him that they might have different aims. By what we call "putting ourselves in another man's position" we do not claim to be able to predict what he will do (though we may be willing to say what he *might* do), but, when he has done it, we do sometimes seem to ourselves to understand *why* he did it (in a sense apparently incomprehensible to Mr Gibson but familiar enough to most of us) and, though it is very easy to be mistaken about this, we are not always mistaken and there is no other way in which this understanding can be had. Nor is this useless speculation, for it is just what a Minister of Labour wants, who wants to know why there is labour unrest in the docks—not what one of Mr. Gibson's "social scientists" could tell him of the "laws" governing the social behaviour of dockers, but just what it is that these men really want, which might be something which they could quite easily be given, which would satisfy them.

The second half of the book is entitled "The Logical Peculiarities of Social Enquiry", but in fact it is not concerned with social enquiry at all, but only with its anticipated results. The question is not what sort of evidence there can be for the conclusions of "social science", but only what sort of logical system such a science would be—or rather, I suppose, is, for it is taken for granted that we have it. The general picture of Science presented is a conventional simplification of John Stuart Mill. It is all subsumption of empirical generalizations under wider generalizations. Mr. Gibson might almost have been invented by a German sociologist as a larger-than-life caricature of *angelsächsischer Positivismus*. He really believes that we have satisfactorily explained why anything happens whenever we have said that such things always (or even only usually) happen. Doubts and difficulties about the correctness of traditional accounts even of the physical sciences have never touched his mind. The suggestion might be put to him that, even if it is true that in physics we have to be content with empirical generalizations, the reason of that could be that we are not ourselves atoms or electrons, but in the "social sciences" we might hope for something more, because we are human beings.

A. M. MACIVER

*Philosophy and the Modern World* By ALBERT WILLIAM LEVI Indiana University Press, Bloomington, 1959 Pp xiv + 591. \$7 50

THE title of this book is misleading, as the author himself almost admits in his preface, to the extent that he is not concerned only with "philosophy" as understood in academic lecture-rooms. His net takes in Freud and Lenin, Planck and Toynbee, as well as Bergson, Dewey, Russell, Carnap, Sartre and Wittgenstein. At first he speaks in his own person, outlining what he conceives to have been the problems confronting human thought in a period laid down as running "from the publication of Bergson's *Time and Free Will* in 1889 to the publication of Wittgenstein's *Philosophical Investigations* in 1954", and at the end of sixty pages one reader was groaning at the sight of the mass of print which he was under a reviewer's obligation still to read. Here is every platitude on the theme of the contemporary intellectual situation served up again stale and cold—all the things which seemed so illuminating when we first heard them fifteen or twenty years ago, of which we have long since begun to wonder whether they are even true, let alone important. But thereafter, in what is the main body of the book, Mr. Levi makes himself the mouthpiece of twelve selected "prophets", and here what creeps in as a grudging respect finally becomes something like admiration. Mr. Levi is a ventriloquist who can speak in every voice. (Though he does not only expound, but criticises, it is in each case like the gentle criticism of a disciple, even if often of one who is not ready to go all the way with the master.) I do not mean that he represents the thought of his various "prophets" accurately. I find that he often misinterprets, at least in matters of detail, those whom I know well, which makes me suspect that he equally often misinterprets those whom I do not. But only equally—not more or less. It does not shine out—as it would if most of us tried to write such a book—that there are some whom he understands well and others whom he does not understand at all. He seems equally at home with the physicists and with the

psycho-analysts, with the Marxists and the Existentialists and the Logical Positivists. To anyone seeking "orientation" in the world of contemporary thought this is a book which could well be recommended. I myself have certainly learnt things from it about men who were very little more than names to me before.

But there is one exception to this rule of impartiality. Mr. Levi is (as might have been guessed from his imitative title) a devotee of Whitehead to whom he gives the last chapter of the book, as the man who has answered all the questions which troubled the other thinkers of the period. This is wrong. Mr. Levi is entitled to his opinion—and may be held to have made out a case for it, to the extent that his presentation may make a reader think that perhaps after all Whitehead deserves to be considered more seriously than he sometimes is—but it is not generally accepted. It may be doubted whether Whitehead has any place in this book at all. He has not in fact influenced the thought of this period in the way in which all the other men whom Mr. Levi discusses have even if, as Mr. Levi believes, he ought to have done so. And if he belongs in the book at all, it is not at the end but in his chronological place, with Dewey and Russell, before Wittgenstein and the Existentialists. It is in fact serious misrepresentation to suggest that the thought of this period comes to a focus, whether in Whitehead or in anyone else. It is historically false to represent it otherwise than as breaking off in a tangle of loose ends.

A merit of the book which is due not to the author but to the publisher or the printer should also be mentioned. At the head of each page of the notes are printed the numbers of the pages to which the notes on that page refer. This simple device ought to be, but is not, adopted by every publisher who follows the modern practice of printing what used to be footnotes at the end of the book. It reduces irritation immensely.

A. M. MacIVER

*Free Will and Determinism* By ALLAN M. MUNN. Macgibbon and Kee, 1961. Pp 218 42s

Physicists' attempts to grapple with philosophical problems are often vitiated from the outset by ambiguities in their philosophical positions. There is no ambiguity in Professor Munn's position. He adopts a consistent Berkeleyan analysis of experience throughout his argument; and a consistent view of the nature of the language of science, adopted, as he acknowledges, from Morris and Carnap. This leads him to treat 'material object' as a derived concept, not given in 'raw' experience, and acceptable to us in so far as it enables us to organize our experience into a consistent system. Similarly he treats causal relations as those concepts of causal connection which survive from the infant into the adult Newtonian view of the world; their acceptability being determined by their success in making a consistent system of our experience. However much one might feel disposed to disagree with the philosophical and logical machinery deployed in this book, it has the great merit of being worked out wholeheartedly and, apart from a few insignificant details, consistently.

The central part of the argument is designed to show that we need different a-causal relations to organize the experience of the sub-microscopic world which our new instruments have revealed to us. Here the author's position is more liberal than most physicists, since he is prepared to

allow the logical possibility of further experience which could revive the need for causal and determinist relations. This part of the book is excellent, making the proper distinction between experimental facts, the *ad hoc* laws which were invented to handle these facts mathematically, and the overall mathematical formalisms of quantum mechanics, which were designed to fit these laws into a consistent system. The mathematical expressions and derivations are paralleled on the same page by discursive accounts of the intellectual processes involved, so that someone not mathematically inclined, would, I think, grasp the essentials of the new physics from this treatment.

In the last section Professor Munn discusses three cases, heredity, perception and neural networks, in which quantum considerations become important for a scientist investigating these features of human beings. The inner view taken by people themselves of their own actions which involves the concept of free-will, is not inconsistent with any outer view that might be taken by a scientific investigator, since, according to Professor Munn, one's inner-view concept of free-will can be defined in terms of relative ignorance of the determining causes of one's actions. But, if our view of the world, in general, was deterministic we should have to 'deny that there is any real sense in which the individual can be said to "possess" free-will'. The conclusion drawn from this is a useful one. It is that though affirmation of determinism in general requires the denial of free-will, it is simply a logical mistake to infer that a generally indeterministic world picture requires the affirmation of free-will. Within the rather crude analysis that the author attempts this conclusion seems to be true. But in drawing his further conclusion that the affirmation of free-will requires the affirmation of indeterminism Professor Munn slips past all the difficulties in his Berkeley-Garnap view of epistemology and logic, and begs all the questions which are inevitably raised by the covert inclusion of the individual's language for describing his own actions into the scientist's language for describing the world. This inclusion is a necessary requirement of constructing such conditional statements as 'Determinism implies not free-will'.

R. HARRÉ

*Immortal Longings.* By STEPHAN FINDLAY. London Victor Gollancz, 1961. Pp 192. 21s

THIS book is an attempt to inspect the various arguments for survival after death. It is a popular work of the better type, and at least offers no *glib* solutions. It divides roughly into three unequal parts: a consideration of religious arguments, a recapitulation of the work done in psychical research which might have a bearing on the problem of survival and some Russellesque advice on how to face future death with equanimity. Mr. Findlay's general thesis might be summed up "You're not likely to survive but don't worry about that."

The considerations which Findlay brings to bear on the religious arguments for survival are loose in form and unoriginal in content, but they are also convincing as against a crude Christian view. However, the whole attack is too broad and undirected to be useful philosophy. For instance, the Ontological argument, if it is to be considered at all, needs more than a page or so for any sort of adequate discussion.

Nearly half Findlay's book is devoted to telling us about experiments in psychical research (not including mediumistic experiments which, he rightly points out, can all necessarily be explained in terms of telepathy and precognition) What is never seriously tackled is the logical connection, if there is one, between paranormal phenomena and survival. In one of the rare passages which touches on this matter, he writes, "Since the mind can transcend the limitations which restrict physical objects both in space and (probably) in time, it may be able to act independently of the physical, that is, survive after death" (p 161) But the mind has always been undisputably able to transcend the limitations which restrict physical objects, in Findlay's sense, and if we can telepathize or precognize that is just a new way in which our minds do this. From the point of view of evidence for having a mind independent of a brain, what is the difference between predicting future events on scientific grounds and predicting them by precognizing them? Thus it is hard to see that a discussion of telepathy and precognition has the slightest bearing on whether we survive that a discussion of imagining or reasoning would not have I am saying that nearly half Findlay's book is *prima facie* irrelevant to our immortal longings and that he brings no substantial grounds for saying that this *prima facie* view is mistaken

The book contains no index and precise references are not given for all the quotations Far more important, the publishers are entirely silent on who Findlay is In a book which contains a good deal of reporting and evaluating of evidence not fully given (much of the stuff about paranormal phenomena) it seems a serious omission to give the author no credentials whatever

JON WHEATLEY

*L'Homme Machine* By LA METTRIE A Critical Edition with an Introductory Monograph and Notes by ARAM VARTANIAN. Princeton University Press London · Oxford University Press. Pp 264. Price 48s

PROFESSOR ARAM VARTANIAN presents a scholarly edition of La Mettrie's *L'Homme Machine*, which first appeared in 1748, together with a lucid and cogently argued monograph in which he attempts to estimate the force and originality of La Mettrie's mechanist theory and to assess its significance in the materialist tradition. This monograph is primarily an essay in the history of ideas and Professor Vartanian is sensible of the difficulties encountered in locating sources and in assessing the influence of one thinker upon another. In studying La Mettrie these difficulties are aggravated by the fact that much of the materialist literature of the period circulated anonymously and clandestinely and by La Mettrie's own manifest delight in provocative and sensational polemics which served to discourage the prudent from acknowledging their debt to him

La Mettrie is held to have been original in the following respects · He presented a far more coherent and consistent statement of the man-machine doctrine than anything that can be found in the works anticipating it; his scientific interest in materialism dominated the motives of anti-religious subversion, and he used the principle of irritability to arrive at an organicist definition of the machine as a self-sufficient system of interdependent parts. La Mettrie extended the application of mechanist principles from the Cartesian 'la bête machine' to the entire human



organism and maintained that the correlation of mental and physiological states would prove sufficient to provide a comprehensive psychological theory. Thus La Mettrie denied the existence of a mind or soul functioning independently of physiological states and asserted that the whole range of human activity is explicable in terms of physiological mechanism. Professor Vartanian notes that the principle of psycho-physiological correlation was presented in two ways: first, and more blatantly in response to the polemical needs of the time, as an absolute metaphysical principle from which La Mettrie's readers would not be slow to infer the denial of spirituality, and secondly, in a more sophisticated way as a heuristic device or principle of scientific methodology. Professor Vartanian claims that despite the polemics La Mettrie's main concern was with the latter, but in his attempt to remove the metaphysical prejudices which inhibited empirical investigation in psychology he was forced by the lack of an adequate methodology and the need to gain a serious hearing—which in the climate of the time the presentation of his thesis as a hypothesis would not have secured—to state his case in a metaphysical and absolutist manner.

It may be doubted, however, whether La Mettrie was as clear in his mind on the status of his principle as Professor Vartanian suggests that he was. While it is true that La Mettrie explicitly refused to dogmatise on the relation between organic process and the development of consciousness beyond stating the psycho-physiological parallelism, and though he refused to dogmatise on the relation between subjective and objective reality, yet when he discussed the moral and practical implications of his thesis this caution and circumspection disappears and, as in his discussion of happiness in the *Discours sur le bonheur*, his confidence seems to anticipate the complete empirical confirmation of his position and to exceed what may be derived from a methodological principle.

The stumbling block to the development of mechanism along Cartesian lines had been the failure to demonstrate how the organism could possess inherent powers of purposive motion. La Mettrie inferred such a property from the phenomena of muscular irritability and used it to account for the ability of the living machine to move itself immediately and autonomously from within. Professor Vartanian examines in great detail the sources of La Mettrie's thesis. Steno had shown that the heart functioned as a muscle, Leeuwenhoek had seen through the microscope the fibrous composition of muscle tissue, Francis Glisson had speculated that movement is innate to all parts of the body and had used the term 'irritabilitas' for this capacity. Boerhaave had assembled most of the relative evidence, but the more immediate source was Haller who, though his *De partibus corporis humani sensibilibus et irritabilibus* did not appear until 1752, had called attention to the facts and problems of irritability in his edition of Boerhaave's *Institutiones* (1739 to 1743). Haller, however, had confined his applications of the principle of irritability to the muscles, and it was La Mettrie's achievement, Professor Vartanian claims, to have synthesised the experimental proof of muscular irritability and the generalisation of that property to all living systems. Irritability thus finally replaced the soul or vital principle as a concept essential to biology and became the basis for investigating the psychic aspects of the life process. Professor Vartanian supports his interpretation of the text and his assessment of La Mettrie's significance with an erudition which shows a detailed grasp of the philosophy, medical sciences and the movement of ideas in the eighteenth century.

D. O. THOMAS

*Remembering* By W. VON LEYDEN London. Gerald Duckworth and Co., 1961 Pp 128 15s

In the introduction, the author says that his "chief aim is to consider the kind of difficulties that lie in the way of trusting our memories". As a result of his considerations, he claims that we must accept that memory is not infallible, but that we can reject the idea that memory may always deceive us. Despite this, he maintains that even genuine recollection is to some extent delusive. Finally, he concludes that no set of statements about the past entails the claim that we remember some past event and that we never do remember physical events, but only our own experiences.

In very many places Von Leyden gives every appearance of mixing up definition with epistemology. Ryle, Ayer, Benjamin and Holland say that it is part of the analysis of a person's remembering something that the thing did exist. Constantly he criticizes these writers for thus "begging the question" about how we know what happened. In his summary of conclusions, he says that such logical criteria "cannot be made sufficiently explicit for the purpose they are expected to serve, i.e. to vindicate or confute memory claims . . .". It is a gross error to expect definitions to show us whether memory statements are true, but perhaps Von Leyden comes to this conclusion because he believes that "'A remembers that p' entails 'p'" entails that memory is infallible. He says explicitly that Ryle, Holland and others who adopt what he calls the Past Approach believe that memory is infallible, and that memory statements are necessary. Of course memory is not infallible, and he points out some of the interesting ways in which memory may mislead us. But no writer whom he criticizes does believe that memory is infallible. For instance Ryle is at pains to show that although it is a criterion for someone's remembering an event that the event occurred, this does not mean that memory is an infallible source of knowledge. Sometimes Von Leyden recognizes this, but often he does not. For instance he thinks that Ryle's position must oppose the legitimacy of Russell's question whether all of our memories might be mistaken.

Quite apart from this misconstruction of the logical fact about memory claims, the author concludes too quickly that Russell differs from Ryle on the point of definition. I doubt that Russell would say that there could be a question whether there had been a past, if it were known that we remember. The use of phrases such as 'mistaken memories' scarcely even suggests that one may remember an event which has not happened. By definition, a collection of parts is not a motor-car unless they are put together in a certain way, but this does not prevent us from using the term 'disassembled motor-car'.

Von Leyden believes that if there is good reason to be sceptical about memory, then memory of an event cannot necessarily involve the occurrence of that event. Consider how curious his own moderate scepticism would appear if this belief were right. If "A remembers X occurring" does not entail "X occurred", then the former statement may be true even when X does not occur. But if this is so, then no force is left in Von Leyden's own question "How can I know that I remember an event?" If his claim to remember can be true without the event in question having happened, then it is just too easy for him to know that he remembers.

Although sometimes the author is too lax about the criteria for remembering, at other times he is rightly stringent. He points out against

Holland that to remember something is more than to report it, and to have witnessed it. Thus we may wonder whether we remember an event even where we know for certain that the event occurred. However one of his inferences from this fact is quite unjustifiable. He considers that the fact empties of sceptical force Russell's philosophical supposition that the world might have come into existence five minutes ago. He reasons thus. Even if he knows that something has happened within his experience, still he does not know that he remembers it. His problem is to know that he remembers. Even if Russell's supposition is shown to be false, and he knows of any event which has been within his experience, still there is the problem of how he knows that he remembers any event. Therefore Russell's supposition is not relevant to scepticism about memory.

This reasoning is fallacious. What his point against Holland shows is that there are at least two problems to be overcome when we wish to know that we remember. We must know that the event occurred as we seem to remember it, and we must know something else. If we know that *p*, it does not follow that we know that we remember that *p*. So far so good. However if we do not know that *p*, then it *does* follow that we do not know that we remember that *p*. Therefore Russell's supposition that for all we know, there has been no past, is of consequence for scepticism concerning memory. One of Von Leyden's conclusions is that it is not logically possible to be mistaken at all times when we seem to remember. I find no argument except this bad one, to justify his rejection of radical scepticism. He toys with the idea that the additional criterion needed for memory is that the report should be causally dependent on the past experience of the event. Sometimes he says that it is a conceptual truth. At other times he says that it is unsatisfactory to think so, because we cannot finally establish that such a connection exists in any particular case. On page 27 he quite inexcusably imputes the view to Ryle, who quite explicitly rejects it on page 278 of *The Concept of Mind*.

Another of Von Leyden's conclusions is that we never remember a physical event, but remember only our own experiences. A case is described where someone sees a panio in Trafalgar Square, and thinks that it is a setting for a film. Later, by memory, he reports having seen a film setting at that time. The author argues. His report about a physical event is false, but his memory is not at fault. Therefore his memory is not of a physical event. In the space of a review, I can say no more than that the same form of argument would show that people do not see physical events. For while the man is watching the panio, let us suppose that he reports seeing a film setting. His report of a physical event is false. But his sight is in no way defective. Is it supposed to follow that what he sees is not a physical event?

There is a treatment of problems arising from the possibility that we remember our previous memories of an event, rather than the event itself. Also there is some discussion of the fact that what we remember is to some extent determined by our interests and preconceptions both at the time when we perceive, and when we remember. I am not sure that the facts which Von Leyden brings forward establish his final conclusion that *all* our genuine recollection tend to be in part delusive. It is one thing to remember only some things about an event, and another to be deluded about that event. However, no doubt if we base an evaluation of an event only on our own memory of it, these facts which Von Leyden reports, do constitute a source of bias.

Although, so far as I can see, the author fails to carry out the main aims of the book, and to establish his stated conclusions, he makes some valuable points along the way. He maintains stoutly against Ryle that memory can be classified as a way of knowing the past, he is rightly convinced that Holland has omitted some important criterion for memory, and he resists Malcolm's positivistic account of remembering dreams. At the same time I must say that if I held any of these positions, I would not feel that arguments sufficiently precise and detailed had been advanced against me. Russell's name is misspelt in the bibliography.

M. DEUTSCHER

*The World of Art* By PAUL WEISS Southern Illinois University Press, 1961 Pp 182 \$4.50.

THE present work offers a broad philosophical view of art. Professor Weiss moves easily among wide ranges of aesthetic and metaphysical ideas, he accommodates remarkably various approaches in what may or may not be a unitary scheme. Hardly anything respectable gets quite left out. Foremost is the apparently Hegelian statement that art makes "an ideal permeate every part of some material, thereby giving the ideal sensuous form" (p. 7). Elsewhere it embodies "prospects", being "iconic of existence beyond" (p. 116). As to these prospects, they are, I think, states of affairs envisaged or sought. Faced with real possibilities, an artist is one who gets his excessive emotions in order by working them out in sensuous form.

Thus emotions come into the story, and a place is next found for Collingwood. The artist is seen as clarifying his own emotions, and coming to know them by expressing them. He creates for them an adequately structured correlate, and so in turn we embrace some sort of formalist position too. A work of art is not only the expression of emotion and the sensuous embodiment of an ideal, but also a unitary structure, an articulated whole. It is, we read, "self-contained, significant and structured." It makes possible both the enjoyment of the quality of existence and an awareness of existence's import for man" (p. 115). Whether it might be possible to make objects answering to one of these descriptions and not the others, is not, I think, altogether clear. Finally Professor Weiss cogently maintains that works of art have their own ontological status, correlative to human concerns, and are objective after their own kind.

I am not sure how far the doubts I have hinted at are legitimate. Professor Weiss's ideas are not easy to assess, they have an appearance of moving in some plane just beyond ordinary reach. His language is resonant and semi-technical, and he seems averse to calling anything by just the name that other writers have used. "Revelation" theories doubtless have a permanent appeal. And specific aesthetic qualities are inevitably expressed in terms—even "sublimity", "grace" or "sophistication"—whose first use is elsewhere, outside art. But if, say, the Villa Rotonda, apart from being sophisticated, a kind of *capriccio*, represents some reality beyond itself, a philosopher must be asked to argue pretty hard to show us what and how. That is the claim. but for my own part, being sceptical of it to start with, I am little nearer believing it now.

DAVID POLE

*The Fabric of the Heavens* By STEPHEN TOULMIN and JUNE GOODFIELD.  
Hutchinson. Pp 272. 25s

THIS book is about "the development of astronomy and dynamics and the contribution these sciences have made to our cosmological picture". It is the first of a connected series of four volumes on *The Ancestry of Science*. The other three volumes will deal with our conception of material substance, with the way in which the historical dimension entered science, and with the impact of science on the other aspects of human life and society. The series explores the origins and development of what is now our "common sense" picture of the world and it has been written in the conviction that such explorations will be of interest to students of science, history, philosophy and literature. The conviction is fortunately shared by The Nuffield Foundation Unit for the History of Ideas under the auspices of which the series has been written. A film *Earth and Sky* has also been issued in conjunction with the present volume.

The belief that the history of ideas is a subject through which all educated people can increase their understanding of our world-picture is one which is becoming increasingly widespread, and the present volume ought to enhance this belief. The book is both clear and informative and succeeds in holding the reader's attention to the end. The emphasis throughout is on the historical context in which scientific ideas have emerged and developed. We are invited to view the history of science not just as a matter of the slow improvement of techniques of observation and measurement and of the progress from wild speculations to testable hypotheses but also as part of the history of civilisation. It is not merely that earlier scientists gave false or crude answers to the questions that we still ask: the very questions asked were different since they arose out of different ways of life. Thus, the accurate observations and records of the Babylonian astronomers and the explanatory theories of the Greeks can be related to the pre-occupations and prejudices of the civilisations in which they were produced. And Aristotle's physics is for once presented as something rational and demanding respect and his cosmology as an intelligible synthesis of the scientific evidence then available "... one cannot judge a man's stature solely in terms of his long-run success" (p. 93). But although to understand scientific ideas we must see them "horizontally" in their historical context, we must also see them "vertically" in their historical development. The authors therefore stress that science was not invented at the Renaissance but that it has a more or less continuous history. Renaissance scientists were influenced not only by Greek thought but also by medieval scientists such as those of the Parisian or Mertonian Schools. Galileo's contribution was to gather together the scattered insights of his predecessors and to present them in a single coherent form with which real-life systems could be identified. Thus, Galileo's work, like that of Euclid, was not just a beginning but also a culmination (p. 218). Moreover, the history of astronomy and dynamics is traced right up to the present day so that we are encouraged to see our own science not as a final formulation but as one more stage in the "vertical" development of scientific ideas.

Each chapter of the book is followed by a useful reading list and the few technical terms are clearly explained, but perhaps (since the book can be recommended as the basis of a seminar course for both scientists and humanists) these terms should have been collected at the end in a short

**glossary** The lack of a subject index is not serious in this book, but an index of proper names would have been helpful

R S DOWNIE

*The Nature of Physical Knowledge* Edited by L. W. FRIEDRICH, S.J.  
Indiana University Press, Bloomington, Marquette University Press,  
Milwaukee; 1960. Pp 156

Has physics anything to learn from philosophy? Although all seven contributors to this symposium agree that it has, they divide into two camps—philosophers on the one hand, physicists on the other—over the question what philosophy is able to teach. It is not surprising, then, that when all is done the 'gap of misunderstanding' of which they complain has still to be bridged. And in this case, at least, the fault does not lie with the physicists.

F. J. Collingwood (Is 'Physical knowledge' Limited by its Quantitative Approach to Reality?) and G. P. Klubertanz, S.J. (Does 'Knowledge' of Physical Laws and Facts Have Relevance in the Moral and Social Realm?), who represent the philosophers in this discussion, seem to take their cue from the editor's hope that 'some contribution would be made toward an all-embracing view of reality, which may some day serve as a frame of reference for evaluating the scientist's concepts of the real' (p. 7). This is hardly a promising beginning. But worse, they are fixed in their ideas as to what this all-embracing view must be—an Aristotelian-cum-Thomistic one—and see their task, therefore, as that of putting physics in its place. The ideal function of science, both believe, is to discover what are the 'ultimate material causes' or essential natures of things, where 'the nature of a thing' is to be understood as meaning 'the internal constitution of a thing . . . the source of its constant specific tendencies and of the activities proper to it' (p. 75). Yet they appear to differ over the extent to which physicists actually pursue this ideal. Collingwood complains that they do not—by adopting a purely quantitative approach, he contends, physicists have condemned themselves to deal with mere suppositions in place of true explanations, 'hypotheses in place of knowledge of the actual ultimate material elements' (p. 39)—while Klubertanz contents himself with arguing that in so far as physicists are true to their calling, so conceived, what they discover will be relevant to the 'moral and social realm'—in the manner of Aquinas, though more vaguely, he writes 'that is morally good which bears a relationship to a natural need or tendency inasmuch as this relationship is judged by reason and its use is guided by reason' (p. 877). Conceivably physicists might learn something from scholastic philosophy but I doubt whether it is anything like this.

What, then, has philosophy to offer the physicists? The answer that emerges, I think, from the papers of the remaining symposiasts—all physicists of note—is this: philosophers are, or should be, able to assist physicists to greater sure-footedness in attempting to surmount those epistemological, methodological, and sometimes purely logical problems with which they are nowadays so often confronted. To be sure, P. W. Bridgman (*The Nature of Physical 'Knowledge'*) claims at one point that the physicist's conception of reality 'has almost no recognisable philosophical component' (p. 21), but the 'philosophy' that he there eschews is of the kind represented above for the most part, Bridgman, like R. J.

Seeger (*Metaphysics Before or After Physics?*), is plainly aware of conceptual difficulties in physics and of the need for an application of philosophical techniques of analysis to clear them up

For more concrete examples of these conceptual difficulties and the ways in which philosophical insight can help towards their solution, one must turn to the papers of H. Margenau, A. Grunbaum, and A. Landé. Margenau (*Does Physical 'Knowledge' Require A Priori or Undemonstrable Presuppositions?*) discusses the logical status and pragmatic role of certain metaphysical principles of science and shows, by way of illustration, how by satisfying the 'principle of multiple connexions' the neutrino, which 'started out as a metaphysical gleam in Pauli's eye', has been accepted into the ontology of microphysics. Grunbaum (*The Role of A Priori Elements in Physical Theory*) examines the logical status of physical geometry in order to give point to his logical critique of Duhem's thesis that attempts to falsify isolated hypotheses are inevitably inconclusive, and Landé (*Dualistic Pictures and Unitary Reality in Quantum Theory*) exposes the 'double-or-triple-think' of those contemporary physicists who, while they 'accept Born's clear and realistic unitary particle interpretation *also* do homage to the dualistic and neutral doctrine of Bohr and Heisenberg, occasionally also relapsing into the ideology of unitary wave theory' (p. 89)

Now although these three physicists have performed their epistemological and logical critiques well, there remain—especially in the area of quantum physics where, as Landé has elsewhere put it, 'anything goes'—many questionable presuppositions and plain confusions in the detection of which the talents of a hard-headed philosopher might prove invaluable. True, one does not have to be a philosopher to see that there is a contradiction involved in asserting both, say, that a microphysical 'particle' does not *have* either position or momentum and that it is possible to determine with whatever degree of accuracy we choose either the particle's position or its momentum though not both. But neither does one have to be a physicist. And the fact is plain to see that such inconsistencies have bedevilled the philosophical reflections of quantum physicists and so contributed to the present gap of misunderstanding. They have gone unnoticed by the physicist either because he is philosophically inept or because his daily concern is with more practical matters and they have gone unchallenged by the philosopher either because he is ignorant of physics or because he remembers with embarrassment the attempts of his elder colleagues to do physics *a priori*. In these circumstances, it is far from platitudinous to suggest, as does Margenau in the recorded discussion at the end of this book, that 'by trying to make things more precise, by trying to show the philosopher what the physicist means by his terms, and, reciprocally, by inducing in the physicist an attitude which inclines him to listen with attention and understanding to what the philosopher is saying, we are going to bridge one of the deep crevasses that divides and bifurcates our culture' (p. 135)

R. D. BRADLEY

*The History of Scepticism from Erasmus to Descartes* By RICHARD H. POPKIN. Van Gorcum & Co, Assen, 1960. Pp 236+xvii

PROFESSOR POPKIN tells an interesting and comparatively little known story well. Philosophical scepticism was a natural accompaniment of the

Reformation, but, he maintains, the particular form taken by sceptical argument in this period was largely determined by the dissemination of the works of Sextus Empiricus, which began with a printing of a Latin edition of the *Hypotyposes* in 1562 and culminated in the publication of the complete Greek text in 1621. Not, of course, that he was entirely unknown before. The existence of the manuscripts mentioned by Professor Popkin on page 17 shows that he was known to *someone* at least as early as the end of the thirteenth century, but the extent and nature of his influence in the Middle Ages is unknown and probably now untraceable. His influence in the sixteenth and seventeenth centuries is obvious and acknowledged. La Mothe Le Vayer called him 'le divin Sexte' and Bayle asserted that modern philosophy began with the re-introduction of his writings. Their excitement may seem rather extraordinary to us, but this is probably because we are familiar with the Pyrrhonian arguments as more elegantly presented by Hume. During the period from Montaigne to Descartes, the influence of Sextus was both liberating and profoundly disturbing. Sceptical arguments threatened both dogmatic religion and nascent science. The position of the first was hopelessly undermined. Defenders of the rival faiths adopted these arguments for use against each other, but they were double-edged weapons, and each side was driven to take refuge in a fideistic position. Mersenne and Gassendi, however, by adopting a kind of pragmatism, showed how the reliability of scientific beliefs might be defended against Pyrrhonism. Finally Descartes attempted to build sure foundations for both science and religion by constructing and demolishing a 'super-scepticism', but ended by asserting a fresh dogmatism.

Professor Popkin's book is salutary in reminding us of the importance in the history of thought of figures often regarded as 'minor'. We think of the seventeenth century as dominated by Descartes, but perhaps to his contemporaries Mersenne, Gassendi, or even Herbert of Cherbury seemed equally significant.

M KNEALE

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# MIND

## A QUARTERLY REVIEW

### OF

## PSYCHOLOGY AND PHILOSOPHY

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### I.—MENTAL CAUSES AND FEAR

BY JUSTIN GOSLING

IN her book *Intention* (especially §§ 9-11) Miss Anscombe has some remarks to make about certain feelings, bodily movements and such like of which the subject may know the cause without observation. She is not particularly interested in them except to clear them out of the way, and indeed considers them, at least for her purposes, as of little importance. By "knowing without observation" Miss Anscombe seems to mean a combination of two points: first, that if I claim that it was that alligator that frightened me, it is absurd to support this by saying, "but I noticed it doing it"; and secondly, that it is also absurd to support it by saying that I have noticed a regular concomitance between observation of these objects and the occurrence of these feelings, so the alligator is probably the cause on this occasion, too. As it stands this sounds true enough; but if so, then it is an important distinguishing fact about such feelings, and more understanding of it should throw light on the kind of feeling concerned and the sorts of fact the relevant concepts have to cater for.

In what follows I shall take Miss Anscombe's remarks as a starting-point, and as it is her main example, and also to avoid confusion by variety of examples, I shall confine myself to the case of fear. To begin with I shall elaborate, through some objections, her distinction between cause and object. This will bring out certain connections between fear and learning which help, I think, to explain to some extent this knowledge without observation. It will also involve a discussion of the concept of 'fear' sufficient to suggest certain points about the sense in which it is a feeling, and the principle of individuation of it and certain similar feelings

*Cause and Object*

When she discusses these cases in § 10, Miss Anscombe says that it is important to distinguish between saying that something is the cause and saying that something is the object of a person's fear ; for while the same thing may be both, it is not necessarily so, and is not the cause in virtue of being the object. To illustrate this Miss Anscombe cites a case of a child noticing a bit of red stuff on his way upstairs, asking what it is, and mis-hearing his nurse as saying that it is a bit of Satan. This frightens him. What he is frightened of, of course, is the bit of red stuff, but the cause of his fright is the nurse's remark. In this case object and cause are clearly distinct, though in a large number of cases as, *e.g.* a hideous face appearing at the window, the same thing is both.

This distinction between the cause and the object is one which seems to me, as it stands, extremely obscure, and as I think some interesting points arise from its obscurity I shall now examine it further. As a description of what one would say in the circumstances, Miss Anscombe's account is doubtless right : if asked, not what was frightening the child, but what had frightened him, one would be inclined to refer to the nurse's remark rather than the piece of satin ; whereas in the case of the hideous face, the face would be given as what had frightened him and also as what he was frightened of.

I now propose, for my own purposes, to alter Miss Anscombe's example, so that the child overhears his nurse telling someone, as he thinks, that she has left a red bit of Satan on the stairs. This seems to him a terrible thing to do, and he hopes that it will not be there still when he goes to bed ; but he has other things to distract him before then, and by the time bedtime comes he has forgotten about it, until, as he turns the stair, he sees the red satin and runs downstairs again in terror. If asked now what had frightened him it seems to me that the obvious answer is the red satin, though the reason why he is afraid of that is doubtless the remark he had overheard. Now in the role of explaining the child's fright it is difficult to see what difference of importance there is between the part played by the nurse's remark when it is made at the time of his seeing the satin, and when it is made earlier. In either case it comes in to explain why he is frightened of the satin. But in both cases it is the satin that is frightening him, though in both cases either the satin or the remark *may* be cited as in different ways the cause of his fright. What, in Miss Anscombe's example, inclines one to pick on the nurse's remark, is that, of the new things in the child's experience at that

moment, that is the one which stands out as changing the situation. up till then the child was quite happy about the satin. Whereas in the altered case the child was all ready to take fright at anything red on the stairs, so that his seeing the satin is what stands out as significant. This is what explains one or other being selected as *the* cause; but in both cases both causes are present, and each has its different part to play in explaining the child's fright, in the second case, no more than in the first, do the two fuse.

This last point is of importance because of the example of the hideous face at the window. Of course, this would be what we should naturally give as the cause of our fright, but not because we were fusing cause and object, but because *this* is the significant factor in the situation. Even here, however, there is the possibility of explaining why I was frightened by the face: perhaps I had been looking at too many of Dore's illustrations of the *Inferno*, and that was the cause of my fright. Either may be called the cause, but they remain different kinds.

In short, the nurse's remark explains the child's fright in explaining how he comes to be frightened by *this*; and it is selected as *the* cause here, not because there is only one kind of cause involved and this is it, so that cause is here distinct from object; but because, of the various causes citable, this is the significant one. The case of the hideous face is not by this example shown to be a case where cause and object are one, though logically separable, it may just be a case where the object, always a cause, is also the significant one, though there is also a cause similar to the nurse's remark.

The nurse's remark, as I have said, explains the child's fright by explaining how he comes to consider this piece of stuff frightening. In the example it is hard to think how he could fail to know how he came to think it frightening, and so to know this cause of his fear. This is, however, clearly something peculiar to the case that he learnt to consider it frightening as a result of information just given. But as often as not we may well be at a loss how to answer this question, even if we know why we are frightened in that we are able to say what is frightening us.

There is, however, a distinction which we commonly employ between what has frightened us and what we are frightened of, practical familiarity with which inclines one towards Miss Anscombe's distinction. Thus suppose that I am riding across the western plains and notice intermittent clouds of smoke rising from a hill to the right. This sight might well frighten me; but if so it is not, of course, because I am frightened of the smoke,



but because I am frightened of the Indians of whose presence, and interest, the smoke is a sign. Thus what frightens me and what I am frightened of may be distinct. But clearly also they may be the same thing: what I notice may be the actual Indians, and then the frightening thing I notice is the same as what I am frightened of, so that if we decide to call the first the cause and the second the object, we may say that while the object is often, not to say usually, the cause, it is not always so. Giving the cause, in this case, is giving something that we have noticed which has made us frightened; sometimes what we have noticed is in itself harmless, sometimes itself the source of danger; either may come in indifferently to answer 'What have you noticed?' though there will be different answers in the two cases to 'Why does it frighten you?', so that as one sort of cause they are on a level, even if as another they are not.

If we return now to my example we can roughly distinguish four sorts of causes of my fright. Suppose, then, I notice the smoke signals and am frightened, it may now be said (a) that it was the smoke signals that frightened me, (b) that it was the Indians, of whose presence the signals were a sign, that frightened me, (c) that it was what the Indians, of whose presence the signals were a sign, might do to me, that frightened me, or (d) some explanation might be given of how I came to notice the signals, or to learn that they were a sign of the presence of Indians, or that Indians were dangerous, and one or other of these may be given as the cause of my fright. Thus my guide may have drawn my attention to the signals, or I may have been told that smoke always meant Indians, or have read horrible stories of what Indians did to those they caught, so that the cause of my fright may be said to be the guide, my informant or the stories, as the case may be. It is not hard to see that no very clear limits can be set to what is required to make one or other of these explanations adequate.

The first three kinds of cause are related in that I am only frightened by the signals because I am frightened of Indians, and I am only frightened of Indians because of what they might do to me, it is this that "really" frightens me, and that I am "really" frightened of. It is only because we have things of this sort to be frightened of that things that we notice, whether signs or sources, frighten us at all. But this gradation does not continue to (d). However much the stories I have heard explain my present fright, it is still not they but what the Indians might do to me that really frightens me—and similarly with the other possibilities under (d).

With regard to (*d*), the answers to these various questions may themselves be various. Thus my guide may have drawn my attention to the signals; I may have learnt that Indians are dangerous from first hand experience, and learnt that smoke signals are signs of their presence from films. But also they may all run together. my guide may draw my attention to the smoke, explain to me that it is an Indian signal and go on to tell me how dangerous Indians are; so that my guide's remarks are the explanation of all these factors. Further, sometimes that which comes as the answer to what explains in one of these ways also comes as the answer to what explains my fright in one of the first three ways. Thus the smoke signals may be not only what I notice that frightens me (*a*), but also they may be so noticeable as to explain my noticing them (*d*): they draw attention to themselves. But clearly, to use Miss Anscombe's words, the object of my notice is not as such the cause of my noticing it. It is not difficult, however, to find a great many examples of things which are both object and cause of my attention in this sort of way. When it comes to finding examples where what I am frightened of at the moment (*b*) is also what I should cite as the explanation of my being frightened of it (*d*), on the other hand, one really has to search the by-ways. Possibly an example of this would be my being tortured by the Indians, whom I had always believed to be gentle, when it might be said that the Indians' activities were both what I was frightened of and the explanation of how I come to be frightened of them. Even granted this example, however, unless I have a very short memory the fusion cannot be a common one. if I learn my lesson on the first occasion, the fusion will not recur on future ones. Only rarely is the object of fear also the cause in the sort of way in which the nurse's remark is

To sum up so far there are two fairly common cases of what might be described as a fusion of cause and object. First that which one notices which frightens one may also have attracted one's notice, so that the cause and object of one's notice may be the same thing, although a thing does not attract one's notice to itself in virtue of being that to which one's notice is attracted. Secondly, that which one notices which frightens one may also be what one is frightened of, but it is not what frightens one in virtue of being what one is frightened of, nor *vice versa*. What frightens me need only be something which constitutes a reason for believing in the presence or approach of something which is a source of danger. This fusion is the one which makes Miss Anscombe's distinction attractive. But the nurse's remark is

what *instructs* the child and in this way explains its fear. It is in fact difficult to find examples where the object of one's fear also supplies this instruction.

At this point it may be worth observing two things which perhaps add to the attractiveness of Miss Anscombe's distinction. First, if the common fusion of what one notices that frightens one (*a*) and what one is frightened of (*b*) is in the background as what is being got at, then it is tempting to take the nurse's remark as what one noticed, and so the example as an example of these two things being distinct. After all, the nurse's remark 'constitutes a reason for believing in the presence of something which is a source of danger'. Some differences, however, are worth noticing. With things which one notices which frighten, their continuance either shows them not to be signs of danger (as might be the case with a movement of the door. if not repeated one suspects the worst; if repeated it might be obvious that it was not properly shut and the wind was blowing it), or else it constitutes a continued reason for being frightened, as perhaps with the smoke signals. But if the nurse goes on repeating that it's a bit of Satan, this does not go on frightening the child, nor constitute a continuing reason for being afraid. It is the *satin* that frightens him as a result of her remark—the Indians, by contrast, do not frighten as a result of the smoke signals. This is not to say that remarks cannot be like smoke signals: a look-out in an observation post might serve a very similar function; but the situation has to be changed to enable them to. One does not learn the nurse's remark as a sign, nor is she serving as a look-out. She is answering a request for information.

The second thing which may subconsciously help us to accept the distinction without question is that we may assimilate the case to one where the nurse tells frightening stories, where the stories are clearly what frighten the child. Cases that fit this specification may, of course, vary a great deal. Thus the nurse may here and now elaborate, if she is malicious, on the terrors of Satan, or alternatively she may just be telling horrible stories about Satan without connecting them with any particular things in the child's experience. In so far as it is the first kind of case, however, the child gets an increasing fear of the *satin*, it frightens him more and more, as the nurse elaborates her information, and so what has been said earlier will suffice. In so far as it is the second kind of case it is not clear that there need be any object of his fear in any serious sense; for it is not necessary even that he should believe in Satan, so long as his imagination is sufficiently stimulated; and from this sort of case there is no fear of confusion

of cause and object. What the story does is, either put the child in a state of nerves where anything might frighten him, or give him imaginative thrills of terror. The cause of this is certainly not what he is frightened of; but when might such a cause be what one was frightened of?

### *Fear and Learning*

The purpose of examining the various sorts of answer to the question 'What was the cause of his fright?' was to throw some light on how, if at all, it is that a person can know the cause, and that without claiming to observe it producing the effect. With regard to some of the explanations outlined above it is often the case that I do not know the cause. Thus I frequently do not know the exact circumstances by which I have come to believe that a given thing is a source or sign of danger; and if I discover, then I know them not indeed by observation, but at any rate only as a result of enquiry. With regard to some of the other explanations, on the other hand, it would at first sight seem very peculiar to suggest that I often do not know what I am frightened of, or what is frightening me. On the contrary, it would seem the exception if I did not; and it certainly seems absurd to suggest that I know what is frightening me because I had spotted it doing it, as I might know what was tickling me by noticing someone at work with a feather. Further, as a person is taken usually to know what is frightening him, he is consequently usually taken, granted he is trustworthy, as something of an authority on the subject. The question is, then, how is it that a person can know, straight off, that it is this thing rather than that that frightens him?

Among the explanations sketched out earlier some explained either how I came to notice something, or how I came to believe that what I noticed was either a likely source of hurt or harm, or a sign of the presence of some such source. In these latter cases my fright is explained by showing how I came by certain beliefs, or, granted that I am right, how I learned, say, that smoke-signals were a sign of Indian interest, or that Indians tended to show their interest in unpleasant ways. If these facts explain my fear, then the suggestion is that if there were no such facts something would be left unexplained, the case would be a puzzling one. Usually, if we do not know how we could have acquired the requisite learning, it is assumed that there must have been some experience, forgotten no doubt, which would explain the present reaction. Evidence that there could have been no such experience would

leave one with a problem: how can I be afraid of this thing, which I have no reason to suppose to be dangerous?

Now such puzzling cases do occur, but my point is that they are puzzling precisely because all possibility of learning about the relevant danger is ruled out. If it were ruled out in all cases; that is, if the question "How did X come to consider this dangerous?" was deemed universally irrelevant, and so no answer necessary; then the concept of 'fear' as we have it would disappear. Normally a person's being afraid is a manifestation of his learning about danger; granted that this is generally true, queer cases can be catered for, but they are catered for against this background. Further, I think the notion of learning about danger and of discriminating between what is dangerous and what is safe is inseparable from the idea of wanting to avoid dangers and having some knowledge of what, in some cases, would avoid it. Complete absence of any sign of this would be evidence of indifference to danger, and so of lack of fear. Not that a being has to be capable of learning to avoid every danger, which would be a ridiculous thesis, nor that it always has to give in to its fears and try to avoid it; but it must show itself capable to some degree of learning to identify dangers and avoid them, and with a tendency to apply this learning in particular cases. The range of fears in various subjects will, of course, vary enormously with their abilities; my point is just this: deny all possibility of such learning to a subject, and you deny that fear is attributable.

The point of the above remarks is this: with animals it is a general, but not invariant, rule that when they are afraid they have noticed something which they have learnt to be a sign or source of danger, whose effects they want to avoid. To discover what is frightening them is to discover, normally, what there is within the range of their senses that they could have noticed, and which they could reasonably be expected to take as dangerous. If there is more than one thing, then the question is only answerable, if at all, in favour of one rather than the other, if the animal clearly pays attention to one rather than the other, and reacts in ways relevant to the one menace but not to the other, and so on. With men it is a general but not invariant rule that they can say what they noticed and took to be dangerous, for most men have learnt the terminology of fear and danger and how to apply it to report the dangers they notice. It is not, however, an invariant rule: a person may react on the instant and have no idea what had startled him, and in such cases there is a strain about noticing one is inclined to say that he *must* have noticed that movement of the curtain out of the corner of his eye, and yet it seems that

he did not notice anything himself—he is puzzled by the whole affair. Still, in general a person is expected to be able to say correctly what he has noticed.

Thus it is normal that when a person is frightened there should be something he has noticed (or thinks he has) which he takes to be dangerous, and that he should be able to say correctly what it is. This suggests that the question 'What is frightening you?' at least in the kind of example so far treated, is a question as to what you have noticed that you take to be either a sign or a source of hurt or harm, and a person may in general be expected to know the answer to this straight off.

### *Knowing without Observation*

Thus, however, does not yet answer Miss Anscombe's case. For it is only relevant to the question why I am expected to know what I have noticed that I take to be dangerous; it does not at all explain why I might be expected to know that that is the cause of my jumping back, or having butterflies in my stomach, or sweating. Yet I know it is responsible for these effects without observing it produce them, or as a result of noticing a concomitance between the presence of this object and these effects.

Here, I think, it is important to make certain distinctions, even if a hard and fast line cannot be drawn. For with regard to certain sensations, such as butterflies in one's stomach or a slight constriction of the throat, we just discover that they are common but not invariable concomitants of fear. Most, if not all, occur in other contexts also. But if I do have a slight constriction of the throat, and I have noticed some danger, then the constriction will not seem to need any further explanation. If I am faced with an alligator, I know the sight of it is responsible for the feeling of constriction in my throat, not, indeed, because I can observe the alligator producing the effect, nor because I have in general noticed the observation of alligators to be accompanied by this feeling, but, because I have noticed this feeling is typical when I am afraid, there is no need for further explanation of it, since I know I have noticed the alligator which I take to be dangerous, and consequently know that I have something to be afraid of. That is to say, the occurrence of the sensation is held to be not in need of explanation, not to be odd, granted that I am afraid; but what is known straight off is not that noticing the alligator caused the sensation, but that the alligator is the danger that I have noticed.

On the other hand, suppose that immediately I spot the alligator I take to my heels. Did the alligator make my legs move?

Certainly my flight is the direct result of my noticing it ; certainly, too, I probably know just why I am running without recourse either to observation or enquiry ; also, running is not something which I have noticed often to accompany fear, it is, in this case, what constitutes giving way to my fear. I say 'in this case', because sometimes standing stock still or sometimes stamping on something (e.g. an unpleasant insect) is what constitutes giving way to my fear, that is to say, there is not any uniform activity which constitutes giving way. Yet while I know well why I am running it is not true that I am doing it intentionally, my fear ran away with me and I had no time to think. How, then, do I know that it was the sight of the alligator that made me take to my heels? And here it must be said that perhaps I do not, perhaps I am in such a panic, have so lost my head, that I just do not know what I am doing or why. At the other extreme, it could be that I decided on consideration to run away because it frightened me. But between are cases where I knew all the time what I was doing and why, but I did what I did without thinking, because I was overcome with fear. The fact is that men, like animals, learn to avoid the effects of danger, without calculation or practice, and sometimes men find this learning going into operation before they have had time to wonder what they should do, and they have to stop the train of activities on which they are embarked if they are to direct their behaviour in accordance with some chosen aim or plan. This conflict between fear and the ease of deliberation is one of the things which leads to fear being classed as a feeling, if I am said to give way to my fears, if they run away with me, then it is being ruled out that my behaviour can be placed in a context such that, for instance, all that I did was a result of deliberation, or that it could have occurred to me to deliberate about it and I could easily have deliberated if it had occurred to me. Nonetheless, fear is *quasi*-intentional : I want, in a sense, to escape the alligator's jaws, but not in any sense which suggests that this is something that I have just decided after consideration to be desirable, and I know why I am running, but again not in any sense which carries with it the above suggestions, but it is *quasi*-intentional in that it is not to be described as a case of the alligator forcing me away, but by relation to my having certain instinctive aims and being able both to recognise when they are threatened and to learn what sorts of action might counter the threat. It is, in fact, a case of my fears running away with me.

In between sensations, such as constriction of the throat or butterflies in the stomach, and full-blooded surrender to one's

fears, there are many possibilities which one would hesitate to explain simply on the lines of the one or the other. Thus finching seems to be not quite full-blooded, and starting at a loud bang even less so, though it might be construed as incipient activity, on the other hand blinking, which is not, as finching usually is, the result of unpleasant experiences, is not, either, like sensations, something which just happens sometimes to accompany fear, when someone quickly moves his fist towards my eyes one is inclined to explain the blinking on as it were fear lines, because one believes blinking to be a defence mechanism; though once one has realised this, that it defends the eyes, blinking may become a part or form of finching. In general, there is a gradation between sensations and some bodily movements which one knows commonly accompany fear, and actions which constitute giving in to one's fear. The former needs no further explanation granted that I have noticed something which I take to be a threat; that is, they are explained not by reference to what I have noticed, but by reference to the fact that I have noticed it together with other facts of context such as that I consider it a danger. But they need no further explanation because they have been observed to be common concomitants. (This is not to deny, of course, that some further explanations may be in point: for instance, a physiologist may have a good deal more to say.) This is not the way with actions which constitute giving way to one's fears: one's knowledge that the sight of the alligator made one take to one's heels does not depend on having observed that this action often accompanies fear. One knows straight off that one is running away in order to avoid the alligator, if, that is, one has one's wits sufficiently about one. So in some cases I know the cause of what is happening: my knowledge of common concomitants of fear; in others through a combination of my ability to discriminate dangers and my ability to manage the terminology. If I am not usually right in these ways then either I am not subject to fear, or I have not learned the terminology, or both.

### *The standard v. the genuine case*

In all that has been said so far I have tended to talk as though a case of fear must be a case of some person or animal noticing something which has been learnt to be a source of hurt or harm (or which is a sign of such a source), and as a result doing something to avoid suffering such hurt or harm. It might then with justice be objected that cases which satisfy these conditions are



only a few out of many possible cases of fear, and so, even if these conditions are sufficient they are far from necessary. Thus, a person or animal may sense some danger, and so be afraid ; but the whole point of sensing danger is that there is nothing dangerous to be noticed. Similarly, being afraid of the dark is not like being afraid of a bull-dog . you do not have to consider the dark an object which is possibly going to hurt you. Most obviously, one frequently does not even begin to do anything to avoid dangers, one merely, perhaps, feels qualms.

All this is, of course, very true, and I have no desire to deny either that these cases exist, or that they are genuine cases of fear. I do, however, think that they in a sense rely on the other kind of case for their possibility ; that genuine cases though they may be, the phenomena there interpreted in terms of fear, could not be so interpreted unless it were true of the subject of them that it was capable of learning of dangers and their sources, and also that it was capable of learning some of the things necessary in some cases to avoid the hurt or harm in question, and tended, when faced with danger, to react immediately in a way somehow relevant to avoiding the evil effects. If one or other of these general facts is denied of the subject then the whole structure collapses, and fear cannot be attributed at all. If the subject is incapable of learning, or is such that its learning is never automatically put into practice irrespective of decision, then fear is not attributable to that subject.

Thus the attempt to describe a subject which only senses danger and is never otherwise afraid is headed for failure. The expression 'to sense danger' is devised to cater for an admittedly peculiar kind of case where a person or animal is distinctly uneasy, where there is in fact some danger in the vicinity, but where there is no possibility of the subject's having learnt of the presence of this danger in any of the recognised ways. It must further be true either that when he or it learns of the presence of the danger he will recognise it as such, or that he will come to recognise it as such when he suffers its effects. These last two conditions require that the subject be capable of learning of dangers in other ways than sensing, and indeed unless they are fulfilled the suggestion that the subject has any appreciation of danger at all is vacuous. If they are fulfilled, however, then it is no longer true that the subject only senses danger. Thus while cases of a person's sensing danger may be proper cases of his being afraid, they are catered for against a background of what I have taken to be normal cases

Again, with things like fear of the dark, the point here is that

the dark is feared because it renders the subject vulnerable, unable to observe adequately possible threats. If it were not anyway as a rule capable of discriminating to some extent between those things which are a threat to it and those which are not, there would be no justification for calling its behaviour in the dark fear rather than, for instance, fumbling, nor its trembling fear rather than a phenomenon peculiar to it: night-shivers.

It might be objected that this is to treat fear of the dark as though it were a simple phenomenon. I should admit, therefore, that this is not quite so. A child's resistance to going up to bed alone may be taken as fear of the dark, but so may a person's general nervousness in the dark, his liability to jump at almost anything. It is difficult here to draw hard and fast lines between cases where a person has some specific fear, cases where he is trembling all the time because he does not know what to expect, and cases where at the least sound or movement he jumps a foot. In the first case he is frightened, in the last nervous, but what of the second? One thing remains true, however: nervousness is an explanation that belongs to the fear family, and no amount of jumping, shrinking or squealing will show nervousness in a subject which is shown in general to be incapable of distinguishing the dangerous from the safe. There are, perhaps, people whose nervousness reaches such a degree that there is no longer any discrimination of this kind; but these are cases whose previous history did show such discrimination. If it did not, there would be no justification for talk of nervousness.

With regard to the many cases where fear is not given in to, a number of things need to be said, besides that they clearly occur. To begin with it needs remarking that in not being given in to it has been inhibited for some reason: that is, a part of the full pattern is for some reason missing, this is the point of the expression "not given into". Secondly, while it is not necessary that human beings, or animals for that matter, should always give way to their fears, this is not to say that the connection between fear and action which tends to the avoidance of some hurt or harm is just contingent. If that were so, a person could be said to be afraid on occasions, although he had never even begun to act in the appropriate way without calm deliberation (or the possibility of it) even once in his life. This, however, except, possibly, under one interpretation, is a difficult hypothesis, for it is not easy to describe such a subject so that his failure ever to give in or begin to give in can be explained while still giving some sense to the hypothesis.

Suppose, for instance, that one suggested that a person might

have controlled his fears from birth. He must have so controlled them that he never even began, without reflection, to do anything conducive to avoiding danger. Yet in these circumstances it is hard to see what sense can be given to the notion of control. It would seem that some time he must have found himself starting on a course of action on which for some reason he did not wish to embark; to be able to control his fears he must be allowed, except perhaps in the exceptional case I shall mention later, on occasion to get far enough to discover that he is being led somewhere where he does not want to go, and consequently stop what he is doing, that is, control his fears. Otherwise, it is impossible to say what he is setting about doing in controlling them; rather, one is merely postulating something which accidentally never has its way.

The failure to give in to fear, then, cannot be explained by control from birth onwards. One might, then, drop the notion of control. Fear, after all, is a feeling; one can have the feeling without giving away, and there is no good reason why a person should not have the feelings throughout his life without ever acting appropriately. Now if these feelings are to be thought of as physical sensations, such as heart-flutters, then indeed a person may go through his life having these from time to time; but if this is all; if he never shows any signs of unreflective avoidance of any dangers; then, if we take this last seriously, so that he himself is clearly puzzled by the suggestion that he wants, except in a calm sense, to avoid any dangers, there would no longer be any force in saying that he was afraid. He might, indeed, be said to be excited in the presence of what was in fact dangerous. but for the rest, he would be a peculiarity who was never afraid.

The same point holds with such things as trembling and blenching, as distinct from flinching. So long as the feelings or physical signs have no connection, except by chance, with actions, they are inadequate; whereas if they are turned into impulses or such physical signs as flinching, the connection with action is there, and is such that some explanation for the absence of action is needed.

So far I have talked as though I were only taking account of human beings. But the same general point holds with animals. If you describe an animal which never reacts hurriedly to danger; which from puppyhood upwards always quietly takes stock of any given situation, and, if it is dangerous, does just what is needed to put itself out of danger and no more: which is in general unshockable; then you have just succeeded in describing an animal which is cautious but never afraid. This holds good

even if you add in trembling and such like happenings, so long as you keep all suddenness of appropriate reaction rigorously out of the description

Finally, it might be suggested that it would be enough to say that the subject *wanted* to avoid the danger, without bringing in any activity. But as I remarked earlier, any notion of wanting that is brought in must not bring with it any suggestions of having made up one's mind to it, or having calmly contemplated the prospect and liked it. One knows, when one is afraid, that one wants *e.g.* to run away as far as possible (if one does know it), either in the doing of it, or in the difficulty of stopping oneself doing, where the points made about control come to bear.

It may, perhaps, be the case that a given person or animal never comes across anything very dangerous and so never experiences anything more violent than an occasional heart-flutter, but is nonetheless afraid. I feel somewhat doubtful about this case, in view of the various things said earlier, but suppose we grant it. still, the attribution of fear will only hold granted an unfulfilled hypothetical about more serious cases where there will be some reference to action

This brings me to the *prima facie* conceivable case, which may seem to be an exception. Suppose a person brought up in ideal surroundings, always sheltered from any harm or pain, kept well out of the way of anything frightening. You explain to him about fear how people find it hard to undergo suffering or harm, and hard to do things which risk it. You also warn him that you are going to make him undergo some suffering, and you explain how it is normally caused by certain circumstances, how to avoid the effects of such circumstances, and so what he will need to concentrate on doing if he is *not* to avoid the effects but to withstand them. You then make him suffer, and exhort him never to do what is required to avoid its recurrence. Finally you face him again with the circumstances which will result in this suffering. Now granted all these circumstances, perhaps he would be afraid for the first time, but nonetheless control himself; and perhaps similar precautions could be taken for any future occasions. He could control himself because he would know beforehand what to set his mind to doing: for instance, if standing stock still would keep him out of trouble he must concentrate on keeping moving, while if taking to his heels is what would save him, then he must concentrate on staying put.

Quite apart from the example's being far-fetched, however, there are some points to be noted about it. first, it makes no allowance for possibilities of a person's being surprised by some

danger ; secondly, it supposes his being constantly preadvised of the means of avoidance, and so of what he must concentrate on not doing ; thirdly, whether he is afraid or not will depend on whether he finds difficulty in not doing what fear would lead him to do (and, of course, on the kind of difficulty). In short, the circumstances have to be artificially contrived to ensure no possibility of action, and even so the reasons for attributing fear are reasons for supposing that but for deliberate effort to do something else the relevant activity would have been embarked upon.

To sum up this last section · I have claimed that central to the notion of fear are the notions of being able to learn to discriminate between things which are dangerous and things which are not, of being able to learn some steps which are relevant to their avoidance, and of a tendency to act on this learning immediately on observation of the danger. I have tried to support this by suggesting that while not all cases of fear would seem immediately to exemplify these characteristics, they are examples which could not stand alone as examples without presupposing these characteristics. On the other hand, the kind of example which I treated earlier does seem to exemplify these characteristics in a quite straightforward way, and consequently this type of example holds a kind of pride of place among examples of fear. This is not at all to deny that other cases occur, or that they are indisputably genuine cases ; it is merely to say that they are so because they are in one way or another, more or less complicated, connected with the possession of certain characteristics of which other cases are more straightforward manifestations.

### *Fear and Feeling*

The result of all this is that to attribute fear to a person or animal is to connect what is happening with the characteristics mentioned earlier, to explain it as a manifestation of those characteristics. Thus any particular attribution supposes a good deal else about the subject, and the most drastic rebuttal of all would be one which showed that this particular occurrence could not be fear because the subject was incapable of learning about danger and so on. To attribute fear is to bring in as relevant to the circumstances a particular kind of reaction to danger.

It may, then, be objected that fear is a feeling, and that to attribute fear is to attribute a feeling to a subject, and this has been wholly left out of account. If the subject does not feel afraid then he is not afraid, and that is the end of it. Now that fear is a feeling it would be hard to deny, and also, for what it is

worth, it must be admitted that unless this particular case is a case of the feeling then it is not a case of fear. This, however, says very little until it is clarified ; and for this it is necessary to discuss and distinguish three things (i) the relationship of feeling afraid to being afraid, (ii) the question of giving in to one's feelings, (iii) the sense in which fear is a feeling

(i) Once one starts talking of a person feeling afraid the emphasis certainly seems to be on the feelings he has at the moment . if he does not feel anything than *a fortiori* he does not feel afraid As has been remarked already, what he feels may vary a good deal, and doubtless other considerations of context come in to settle the question of whether it is fear, but still if there is no answer to such questions as ' What did it feel like ? ' ' What sensations did you have ? ' then he did not feel afraid It is not clear, however, that he might not have *been* afraid If, walking through the jungle, I suddenly come upon Miss Anscombe's alligator barking at me, I may well take to my heels in fright ; but if asked whether, when I saw it, I felt afraid, I might rightly answer that I had no time to feel anything, the next thing I knew I was running hard down the jungle path. Yet I ran because I was afraid of the alligator. In fact the most obvious examples for feeling afraid are cases where I stand my ground or do not know what to do, or where the danger is anticipated but not yet upon me , the most difficult are cases of my being carried away by my fear.

(ii) Yet these last are the most obvious cases of my feelings mastering me, where I had no choice but to act as they directed me The important thing about the feelings here is not that they are something I feel, but that I act as I do in a context where the supposition that I might have deliberated about it is ruled out. Doubtless, in cases where I keep control, I am aware of the difficulty of not running away and can notice various physical sensations and so on ; but for my feelings to master me, this is not necessary

The upshot of this is that to attribute fear to a subject is not necessarily to attribute a feeling to him in the sense of asserting that he feels something , though if it is not it is to assert something about his feelings, where this amounts to classifying his behaviour as in some way non-deliberative.

(iii) None of this, however, accounts for one's calling fear a feeling, which is attributed on all occasions. For on either of these counts it is a disjunction of possible feelings for as the sensations may vary, so may the actions that the feelings lead to Yet there is clearly a sense of ' feeling ' in which fear is one feeling and jealousy, say, another. As to the use of the word ' feeling '

in this connection, it is no doubt true that the occurrence of physical sensations is one thing which inclines us to it, though the fact that the reaction is not of our own choosing nor the result of practice also comes in. These, however, as has been seen, do not account for fear being thought of as one feeling. The point here would seem to be that the concept 'fear' caters for a fact about human beings, say ; the fact brought out earlier that they can learn about danger and so on. Now it could not be true of them that they were on occasion terrified or apprehensive or in dread, if this fact did not hold for them ; so that terror and apprehensiveness and dread are not classed as separate feelings. It could, however, quite conceivably be true of men that they were capable of recognising other people's success and given to disliking it, and the other things required for jealousy, whether or not they were capable of discriminating dangers and so on. These facts are mutually independent, and so phenomena interpreted in terms of the one or the other are brought under different feelings.

Thus, with regard to calling fear a feeling : the word 'feeling' is doubtless used for a combination of reasons (a) because of the common occurrence of physical sensations, (b) because of the opposition between fear-reactions and deliberation. But if we ask what is the principle of individuation by which fear is to be classed as a feeling, then we must refer to the ability to learn of danger and its avoidance, and the tendency to act upon this learning without deliberation ; and to the independence of this ability-cum-tendency from others. Connection with this is what is being claimed for some behaviour, looks or feelings in every attribution of fear.

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## II.—EXISTENCE, PREDICATION, AND THE ONTOLOGICAL ARGUMENT

BY JEROME SHAFFER

HUME said, "There is no being . . . whose non-existence implies a contradiction",<sup>1</sup> and Kant said, "The predicate of existence can . . . be rejected without contradiction".<sup>2</sup> In making these claims, Hume and Kant intended to bring out a peculiarity in assertions of existence, for they both would admit that assertions that something was (or was not), for example, round might turn out to be self-contradictory, whereas assertions that something exists (or does not exist) could never turn out to be self-contradictory. Now if this is a genuine peculiarity of assertions of existence, then it follows that any proof that something exists because its nonexistence implies a contradiction will be invalid.

A famous example of an argument which purports to prove the existence of something by showing that its nonexistence implies a contradiction is the Ontological Argument, which purports to show that it follows from a particular concept of God that such a being exists, and therefore that the assertion of the nonexistence of God is self-contradictory. Most philosophers have agreed with Hume and Kant that the Ontological Argument is invalid, although it has recently been defended.<sup>3</sup> My own view is that the argument is basically unsound, but I find the standard criticisms totally unconvincing. In this paper I shall show what I take to be the faults in the standard criticisms and then go on to show what I take to be the proper criticism of the argument.

The many versions of the Ontological Argument have in common the following feature: a definition of "God" is given from which, by the use of certain premises, the conclusion, "God exists", is deduced. The Ontological Argument has frequently been attacked by casting doubt upon the acceptability of these premises. I wish to avoid such controversies. I am interested in the move from a definition to an existential statement. Therefore I shall use an argument which brings attention to bear just on that move. This will be the specimen under discussion:

Let the expression, "God", mean "an almighty being who exists and is eternal". Therefore "God is an almighty being

<sup>1</sup> *Dialogues Concerning Natural Religion*, Part IX.

<sup>2</sup> *Critique of Pure Reason*, trans. Norman Kemp Smith, Macmillan, 1953, p. 504.

<sup>3</sup> Norman Malcolm, "Anselm's Ontological Arguments", *Philosophical Review*, 69 (1960), 41-62.



who exists and is eternal " is true by definition, and that entails " God exists ".

This argument purports to demonstrate that " God exists " is a tautology, true by definition. It is not necessary to show that the expression, " God ", really does mean what it is here defined to mean, for the definition is purely stipulative. But given that meaning, it is argued, anyone who denies that God, in the sense laid down, exists has contradicted himself. Only the laws of logic are required to show that he has contradicted himself.

The following short objections to this argument will not do.

- (1) The question is begged from the start by using a proper name, " God ". No, for " God " is not used here as a logically proper name. I have simply introduced an expression into discourse, an expression which is, grammatically, a proper noun.
- (2) Even if we grant the conclusion, " God exists ", that does not imply that there is a God or that something is a God. Why not? In ordinary discourse the expressions are used interchangeably in most contexts. It must be shown, if it is true, that the implication does not hold here.
- (3) Tautologies only tell us about our use of language, about the meanings of our terms, not about what actually is the case. This objection begs the question. Proponents of the argument claim that here is a tautology which tells us about what actually is the case, namely that God exists.
- (4) But then with suitable definitions we could " prove " the existence of a number of things which we know perfectly well do not exist. No, for the things we know not to exist would necessarily be different from the things picked out by our definitions.
- (5) The argument involves a non-sequitur, for the premise is about a *word* but the conclusion is about something different, a thing. No, the definition of the word allows us to prove, by substitution, that the conclusion is a tautology.

The traditional attack on the Ontological Argument consists in trying to show that the definition is inadmissible because it is ill-formed. Kant argued that definitions can consist only of strings of predicates and since " exists " is not a predicate it cannot be a part of the definition. Others have argued that " exists " is a purely formal element, present in any definition of a thing, and therefore could not be used to show that some particular thing exists as opposed to anything else. I shall, in Part I, discuss these arguments in detail and show that none of them succeeds in establishing any impropriety in the definition of " God ". I have proposed. In Part II, I shall show that the argument, although formally correct, does not do what the religious expect it to do. The agnostic will still be able to raise his doubt and the

atheist will still be able to affirm his disbelief, no contradiction arising in either case, even if each accepts the specimen argument under consideration

## I

“ ‘ *Exists* ’ is not a predicate ”

The Ontological Argument purports to show that God must exist because the condition that He exists is a part of the definition of the kind of thing He is. Kant argued that it could not be the case that existence was a defining feature of God or of anything else, because “exists” is not, as he put it (p. 504), a “real” or “determining” predicate, (he admitted that, grammatically, “exists” is a predicate).

What is a “real” predicate? Kant defines it as something “which is added to the concept of the subject and enlarges it” (*ibid*). This is a most unfortunate definition for Kant to use, however, since it leads to contradiction with another important doctrine of his, that *existential propositions are always synthetic* (*ibid*). Synthetic judgments are those which “add to the concept of the subject a predicate which has not been in any wise thought in it” (p. 48), and if existential judgments are always synthetic then “exists” must be a predicate which adds to the concept of the subject, in short, a “real” predicate as defined above. But even without the difficulties this definition of “real” predicate raises within the Kantian system, it is not a very helpful definition, for it represents predicating something of a subject as *revising the concept* of the subject (by enlarging that concept), something only philosophers of a Reconstructionist bent do very often. But I shall say more of Kant’s misrepresentation of predication below.

What argument does Kant give for holding that “exists” is not a “real” predicate, that is, not a predicate which adds something to the concept of the subject? He argues that if “exists” were a real predicate, then in asserting that something exists we would be altering our concept of that something, thereby ending up with a different concept from the one we started with. Since we now have a new and different concept, we will have failed to assert existence of the original subject. Thus if “exists” were a predicate, “we could not, therefore, say that the exact object of my concept exists” (p. 505). But since we obviously can say that, we cannot be adding anything to the concept of the subject when we say that the subject exists, and therefore “exists” cannot be a real predicate.

It is astonishing that this argument has stood up for so long and is still commended by philosophers, *e.g.* by Malcolm (*op. cit.* p. 44). For the argument, if sound, shows that nothing could be a real predicate. Suppose I wish to say that something is red, where "red" is intended as a real predicate. In asserting that the thing is red, I would be adding to my concept of the thing, and hence would be unable to say that the object as originally conceived is red, that "the exact object of my concept" is red. The argument which shows that "exists" is not a "real" predicate also shows that nothing could be one.

The difficulty here lies in an incomplete picture of predication. Kant seems to think that when I say that so-and-so is such-and-such, I must be doing one of two things: either I am extracting the concept of such-and-such from the concept of so-and-so (an analytic judgment) or else I am revising my concept of so-and-so by adding to it the concept of such-and-such (a synthetic judgment). Now which of these two things am I doing when I say that so-and-so exists? Noticing that existential propositions are often justified by an appeal to experience (p. 506), Kant decides that they cannot be cases of extracting the concept of existence from the concept of the subject. So they must be cases of revising concepts; thus, "all existential propositions are synthetic". But this really will not do either, since to revise one's concept of the subject is simply to change the subject of the proposition. Hence in other places Kant concludes that "exists" is not a predicate at all. Kant's vacillation here comes from an overly narrow account of predication. To say that so-and-so is such-and-such is sometimes neither to analyse the concept of so-and-so nor to revise it, but, to put it roughly, to say something about the object conceived of. This use of language is obviously not peculiar to existential propositions. The sentence, "Crows are black", may be used to express a proposition not about the concept of crows but about crows, and when so used would create as much difficulty for Kant, given his account of predication, as "Crows exist". There may well be important differences between "... are black" and "... exist", but Kant fails to bring them out by this line of argument.

Philosophers have tried to express what they took to be the truth in Kant's claim that "exists" is not a real predicate without appeal to that obscure notion of a real predicate. For example Malcolm restates Kant's argument in this way:

Suppose that two royal councillors, A and B, were asked to draw up separately descriptions of the most perfect chancellor they could conceive, and that the descriptions they produced were

identical except that A included existence in his list of attributes of a perfect chancellor and B did not. (I do not mean that B put nonexistence in his list.) One and the same person could satisfy both descriptions. More to the point, any person who satisfied A's description would *necessarily* satisfy B's description and *vice versa* (p. 43-44).

While Malcolm admits that this is not a "rigorous" argument, and "leave(s) the matter at the more or less intuitive level", he thinks it does show that "exists" is very different in character from the expressions which go to make up a description or list of attributes of something. But I cannot see how it goes to show that at all. For it seems to me false that any person who satisfies B's description necessarily satisfies A's. Could not a nonexistent person, say Merlin, satisfy B's description but not A's? It cannot be said that Merlin fails to satisfy B's description (B might have had Merlin in mind when he drew up the list), unless the notion of *satisfying a description* is such that only real things, existent beings, can be said to satisfy a description. But if one uses this rather technical notion, the argument loses its intuitive appeal.

### *Subject-predicate statements as really hypotheticals*

Some philosophers have attempted to bring out the special feature of "exists" which debars it from appearing in a definition in the following way. If we take "Crows are black" as a typical affirmative subject-predicate statement and "Crows are not black" as a typical negative one, then they would claim that the affirmative statement is equivalent to the hypothetical, "If there exists anything which is a crow, then that thing is black", and the negative statement is equivalent to the hypothetical, "If there exists anything which is a crow, then that thing is not black". Now if we hold that existential statements are of the same form, then "Crows exist" would be equivalent to "If there exists anything which is a crow, then that thing exists", and "Crows do not exist" would be equivalent to "If there exists anything which is a crow, then that thing does not exist". But the former hypothetical is a tautology whereas "Crows exist" obviously is not a tautology, and the latter hypothetical is not readily intelligible whereas "Crows do not exist" is perfectly clear. Therefore it is most implausible to claim that existential statements are of the same form as the typical subject-predicate statements, that is, implausible to construe "exists" as a predicate.

I do not find this argument very compelling. It is a mistake to think that the hypothetical expresses the meaning of the typical subject-predicate statement. Notice that the hypothetical must be put in the form, "If *there exists* anything which is . . .". If we allow the antecedent clause to range over nonexistent as well as existent things, then the hypothetical, "If anything is . . . , then it exists", is not tautological and the hypothetical, "If anything is . . . , then it does not exist", is perfectly clear. And therefore the main reason for saying they cannot be equivalent to categorical existential statements has disappeared. But if we take the antecedent in the strong existential sense, then the claim that such hypotheticals express the meaning of subject-predicate statements breaks down. Consider the following subject-predicate statement. "Unicorns are proper subject-matter for mythologists." It is not equivalent to "If there exists anything which is a unicorn, then that thing is proper subject-matter for mythologists", for the former is true but the latter is false. On the other hand, "Unicorns are proper subject-matter for zoologists" is false, but "If there exists anything which is a unicorn, then it is proper subject-matter for zoologists" is true. And these are not just odd cases. There are many other things which it would be true to say of unicorns, if they existed, *e g* that they would obey the laws of physics, be sought after by zoos, be mentioned in books which describe the species of animals, etc., but which are not true of them since they do not exist; and there would be many things which it would be false to say of them if they existed but which are not false since they do not exist. So the purported equivalence of subject-predicate statements to hypotheticals does not hold. The reason is evident enough. If a thing exists, then given the way the world is it will have certain features and lack others, so to say something about a thing is not the same as to say what the thing would be like if it existed. Of course a thing keeps its defining characteristics whether it exists or not, so analytic subject-predicate statements will entail analytic hypotheticals, but in general the equivalence does not hold. Nor does it hold if we understand the hypothetical as that of material implication, for "Unicorns do not have horns" is false but the parallel hypothetical, "If there exists anything which is a unicorn, then it does not have a horn", is true if taken materially, which shows that they cannot be equivalent.

Since it is a popular view that subject-predicate statements are equivalent to hypotheticals, it is important to see how someone might come to such a view. One way would be through the Kantian restriction of subject-predicate statements to statements

about the *concept* of the subject. If this were correct, then the kind of counter-example I used, where the thing referred to by the concept turns out, given the nature of the world, to have some feature could not arise. All subject-predicate statements would be tautologous, either by analysis of the concept of the subject or by revision of the concept of the subject, and in this limited case the equivalence would hold.

But there is a line of argument more plausible to the modern mind which would yield the same result. Suppose one thought that for a series of words to express a meaningful subject-predicate statement, the grammatical subject must refer to some existent or set of existents. Then there would be no difference between attributing a property to a thing and attributing a property to the thing if it exists. Again my counter-examples could not arise. And philosophers have held such a view. Ryle once said.

How can we make propositions about Mr. Pickwick or sea-serpents, given that they do not exist? We cannot. For a proposition is only about something when something in fact answers to the designation in it. And nothing answers to the pseudo-designation "Mr. Pickwick" or "those sea-serpents".<sup>1</sup>

And Broad holds, "*Dragons do not exist* . . . cannot be about dragons; for there will be no such things as dragons for it to be about".<sup>2</sup> Since such propositions are not meaningless, they must be about something, however, and philosophers of this persuasion have offered various candidates for the subject of statements about nonexistents, urging that they are really about beliefs, books, paintings, propositional functions, properties, inscriptions, or story-tellers.

I do not see why statements cannot be made about non-existents. We can dream about them, think about them, and describe them, just as we can wait for them, hope to have them, and look for them. We can mention them, allude to or direct attention to them, and make reference to them. One thing we cannot do, of course, is to *point* to them, and someone who thinks of mentioning, alluding or referring as a substitute for pointing will be puzzled as to how we can point to what does not exist. But if we have not fallen prey to this overly narrow conception of what it is to mention something, then we will not be puzzled about how we can mention something nonexistent.

<sup>1</sup> G. Ryle, "Imaginary Objects", *Aristotelian Society*, Supplementary Volume XII, 1933, p. 27.

<sup>2</sup> C. D. Broad, *Religion, Philosophy, and Psychological Research*. New York, 1953, p. 182.

The most modest proposal along this line is that of Strawson, who abandoned his earlier position that the use of a referring expression in cases where the object referred to is nonexistent is "a spurious use"<sup>1</sup> in which we either "pretend to refer, in make-believe or in fiction, or mistakenly think we are referring when we are not referring to anything" (p. 40). His modified view is that the "primary" use of referring expressions occurs when the speaker believes the expression to refer to some existent, but that such expressions may be used to "refer in secondary ways, as in make-believe or in fiction".<sup>2</sup> But I take it that Strawson would still wish to say that unless we refer to an existent we do not succeed in expressing a subject-predicate "statement", that is, an assertion which admits of truth or falsity. And if he is right about this, then it becomes more plausible to claim that subject-predicate statements are equivalent to hypotheticals, (I do not wish to suggest that Strawson would claim they are equivalent).

But here again it seems perfectly obvious to me that statements which are true or false can be made about nonexistent things. After all, unicorns do have horns, giants are two-legged, and Mr. Pickwick is a most benevolent gentleman. Nor am I engaging in make-believe or story-telling when I assert these things. My grounds for such assertions are quite different from my grounds for claiming that my neighbour is a most benevolent gentleman, but that does not detract from the truth of such assertions.

To summarize, the claim that subject-predicate statements are equivalent to hypotheticals fails, although perhaps a case could be made for their equivalence with hypotheticals in those cases in which the grammatical subject refers to some existent or set of existents. If so, we might introduce a technical sense of "subject-predicate statement" for those statements which are translatable in the way that "Crows are black" is translatable into "If anything exists which is a crow, then that thing is black". For reasons which differ in each case, none of the following would be subject-predicate statements. "Crows are plentiful", "Crows scatter during storms", "Crows vary greatly", "Crows change over the centuries", and "Crows live in our barns". "Crows exist" would not be a subject-predicate statement either, since the requirement that the subject must refer to some existent

<sup>1</sup> P. F. Strawson, "On Referring", reprinted in *Essays in Conceptual Analysis*, ed. A. Flew, London, 1956, p. 35.

<sup>2</sup> *Ibid* footnote (added to original article), p. 40. See also Strawson's "A Reply to Mr. Sellars", *Philosophical Review*, 1954, p. 229.

would yield the result that if it were a subject-predicate statement it would have to be true.<sup>1</sup> No wonder it would only come to the trivial "If anything exists which is a crow, then it exists", if construed as a subject-predicate statement in the technical sense we have given that term. But none of the peculiarities of "exists" which are brought out by saying that it cannot be an element in a subject-predicate statement, in the sense specified, go to indicate any impropriety in framing a definition in which "exists" appears, such as the definition of "God" given above.

*"Exists" as a universal predicate*

When faced with the claim that if "exists" is taken as a predicate positive existential assertions become tautological and negative extential assertions not readily intelligible, most philosophers have decided that "exists" cannot be taken as a predicate. But some have boldly accepted the consequence that "exists" is a trivial predicate, predicable of everything conceivable. Hence the Ontological Argument becomes harmless, for "exists" is a necessary predicate not only of God but of everything conceivable. As recent supporters of this view have put it:

Every conception involves the predicate "exists". Thus not only God's essence but every essence implies existence.<sup>2</sup>

And Hume suggested a doctrine very much like this when he said:

To reflect on any thing simply, and to reflect on it as existent, are nothing different from each other. That idea, when conjoined with the idea of any object, makes no addition to it. Whatever we conceive, we conceive to be existent.<sup>3</sup>

Thus attributions of existence to anything I conceive become tautological, and denials of existence not readily intelligible.

Now whatever the intrinsic merits of this view, it is a most embarrassing one for Hume to hold. For he also wishes to maintain that "whatever we conceive as existent, we can also conceive as non-existent",<sup>4</sup> and thus that "the non-existence of any being is as clear and distinct an idea as its existence".<sup>5</sup> Since

<sup>1</sup> Cf P. F. Strawson, *Introduction to Logical Theory*, London, 1952, pp 190-191.

<sup>2</sup> G. Nakhukuan and W. Salmon, "'Exists' as a Predicate," *Philosophical Review*, 1957, p 541.

<sup>3</sup> David Hume, *A Treatise of Human Nature*, ed L. A. Selby-Bigge, Oxford, 1888, p 66.

<sup>4</sup> David Hume, *Dialogues Concerning Natural Religion*, Part IX.

<sup>5</sup> David Hume, *Enquiries Concerning the Human Understanding*, ed. L. A. Selby-Bigge, Oxford, 1902, p. 164.



Hume uses "conceive to be existent" and "conceive as existent" interchangeably,<sup>1</sup> these two doctrines are flatly contradictory, for the first implies that whatever we conceive we *cannot* conceive as non-existent and the second states that whatever we conceive we *can* conceive as non-existent. Nor can we save Hume by interpreting the second to mean that whatever we can conceive we can believe to be non-existent, for if I cannot conceive of a thing except as existent, then surely I cannot believe in its non-existence.

Perhaps Hume was writing carelessly when he said that "whatever we conceive, we conceive to be existent", and expressed his real thoughts more precisely when he said, "whatever the mind clearly conceives includes the idea of possible existence".<sup>2</sup> But if he did seriously hold the first view, there are many ways in which he might have come to it. The reason he does give,

Since we never remember any idea or impression without attributing existence to it, the idea of existence . . . must be the very same with the idea of the perception or object.<sup>3</sup>

confuses the existence of the conception with the existence of what is conceived, the *realitas formalis* of the idea with its *realitas objectiva*. But he might have argued, in line with his notion of ideas as pictures, that we cannot form the picture of a thing as non-existent, and therefore cannot conceive such a thing. Also his attack on the abstract idea of existence as distinguishable and separable from the ideas of particular objects<sup>4</sup> and his connected claim that in making judgments that something exists we only entertain the idea of the thing in an especially lively and forceful way<sup>5</sup> lead him to say that the idea of existence cannot be something over and beyond the things we conceive, and therefore would incline him to say that whatever we conceive, we conceive to be existent.

Such a doctrine leaves most unclear what negative existential judgments could be, and yet it is obviously most important to be able to give such judgments sense. At one point, when Hume concerns himself with negative existential judgments, he abandons this doctrine, interpreting them as judgments in which the idea of the object is conjoined with the idea of non-existence,<sup>6</sup> although in another place he flatly rejects such an interpretation without giving what he takes to be the correct account.<sup>7</sup> So far as I can see, the contradiction here was one

<sup>1</sup> *Treatise*, *op. cit.* pp 66-67.

<sup>2</sup> *Ibid* p. 66

<sup>3</sup> *Ibid* p. 86 and *passim*.

<sup>4</sup> *Ibid*. p. 96, footnote.

<sup>5</sup> *Ibid* p. 32

<sup>6</sup> *Ibid* p. 623

<sup>7</sup> *Ibid*. p. 15

which Hume was never able to eliminate<sup>1</sup> Nor, so far as I can see, is it possible to reconcile the two. If the definition of a substantive must include the notion that the thing exists (which is what I take Hume's doctrine to mean), then that the thing exists follows from the definition and is necessarily the case. To deny that the thing exists is to contradict oneself just as certainly as to deny that the thing possesses any other defining characteristic is to contradict oneself. And if all our conceptions of things include existence as necessary properties of the things, then no denials of existence will make sense.

Hume was mistaken in thinking that whatever we conceive of we must conceive to be existent. For suppose it is a necessary feature of a chimera that it be not only a she-monster of a particular sort but an imaginary she-monster. Then it would be a necessary statement that chimeras do not exist, anyone who held that chimeras exist would contradict himself. Could one conceive of a chimera<sup>2</sup> I do not see why not. But one would be conceiving of it as nonexistent.

I do not wish to suggest that philosophers were mistaken in thinking that "exists" is in many ways different from grammatically similar expressions, for it obviously is. To take Hume's point, for example, if I wish to picture an animal, it will make a difference whether I picture it as yellow or not. but one cannot make the same kind of sense out of speaking of picturing it as existent or non-existent. It does not follow from this that whatever I picture I picture as existing, but Hume is certainly right in thinking that I cannot represent the existence or non-existence of the thing by adding to my picture in an exactly parallel way to the way in which I represent the yellowness or non-yellowness of the thing by adding to the picture. It requires some special convention to indicate that what is pictured is pictured as, say, imaginary. (Comic strip creators have special conventions for showing this: for example by encircling it and linking it by a stream of small circles to someone's head to show that he is just imagining it.) To make this point is to bring out a difference between "exists" and predicates like "is yellow". Further differences between "exists" and other predicates are brought out in the arguments for the slogan, "Exists is not a predicate". What must be shown, however, is that these differences bear relevantly on the issue whether existential statements can be true by definition.

<sup>1</sup> J. A. Passmore in his illuminating book, *Hume's Introduction* (Cambridge, 1952) diagnoses the difficulties which appear here as arising from Hume's attempt to produce "a logic in which the only links are psychological" (p. 27).

I have been concerned to argue, in this section, that no differences have been noted which rule out existential statements which are true by definition.

## II

Until further arguments are offered, it seems reasonable to hold that there is nothing logically improper in so defining the expression, "God", that "God exists" is a tautology and "God does not exist" self-contradictory. In fact it seems to me that the definition I have given expresses a concept of God (*i.e.*, as necessarily existing) which many people actually accept (just as it is a common conception of Satan that he merely happens to exist). I wish to show in this section that this concept of God can give no support to the religious. I shall argue that no matter what its content, this concept of God is still simply a concept. What must be shown, and what cannot be shown just by an analysis of the concept, is that there actually exists something which answers to the concept. Even if we have here the concept of an object which necessarily exists, a further question remains whether any existent meets the specifications of the concept. The difficulty lies in showing that this further question makes sense, for I have admitted that "God exists" is a necessary statement, analytically true, and therefore it looks as if there could be no further question. But that is an illusion. It must however be dispelled.

As a first step, I wish to point out that the concept of God is hardly unique in its capacity to generate a tautological existential statement. For one thing, we could invent new tautologies. Suppose we introduce the word, "particular", to mean "object which exists", and the word, "nonentity", to mean "object which does not exist". Then to bring out the difference between these two words we might properly say, tautologically, "Particulars exist and nonentities do not exist". Nor, for another thing, do we have to invent such words, for we already have many words with existential notions included in their meanings. The following sentences all have tautological uses. "Existents exist", "Fictitious objects do not exist", "Members of extinct species existed once but no longer exist", "Hallucinatory objects do not exist", "Historical persons have at some time existed". I do not suggest that these sentences can never be used in non-tautological ways, but I do suggest that they may be used tautologically in those circumstances in which we wish to emphasize that such concepts include as a necessary feature, as a defining element, notions of existence or non-existence.

As a preliminary to seeing what these tautological existential claims come to, let us examine the relation between expressions of the form, "A's exist", and of the form, "There are A's". Take the tautology, "Fictitious objects do not exist". One might think that "Fictitious objects do not exist" means the same as "There are no fictitious objects". But a moment's thought will show that this is incorrect, for although the former is true the latter is false. There are fictitious objects, many of them—Alice's looking glass, Jack's bean stalk, Wittgenstein's beetle, to mention only a few. So "Fictitious objects do not exist" does not mean the same as "There are no fictitious objects." Similarly "Particulars exist" does not mean the same as "There are particulars". In general, given a tautology of the form, "A's exist", we cannot deduce from it, "There are A's", nor from a tautology of the form, "A's do not exist", can we deduce "There are no A's". And specifically, given the tautology, "God exists", we cannot deduce from it, "There is a God". The statement, "God necessarily exists, but there is no God", is *not* self-contradictory.

As it stands, the situation is most paradoxical. For in many of its ordinary uses, "A's exist" is equivalent to "There are A's". If I raise a question about the existence of pearls as large as my fist, it usually does not matter whether you say, "Yes, such pearls exist", or "Yes, there are such pearls". So if there is a way of saying that certain things exist which does not mean that there are such things, then this must be explained.

Now I have misinterpreted the situation somewhat. I have spoken as if it were important whether we used the form "A's exist" rather than "There are A's". But this is not the case. "There are A's" is perhaps more resistant to being treated as a tautology but it is still possible to frame tautologies of the form, "There are A's". "There is what there is" and "There are what there are", are tautologies, and one could imagine situations in which one might say them in this tautological way. If I define "particulars" as "whatever entities there really are", then "There are particulars" is a tautology, namely, "There are whatever entities there really are". And if I define "a God" as "whatever divine being there is", then "There is a God" is tautological too. So there is nothing distinctive about the forms "A's exist" or "There are A's". They both function in very similar ways. But we do have both forms of expression, and that is most convenient. For it allows us to formulate a further existential question, using the alternative form, when we are presented with a tautological existential assertion. Thus if

someone says, tautologically, "There are particulars [i.e. there are whatever objects there are]", we can avoid the danger of formal contradiction in asking, "I grant that there are particulars, but do particulars *exist*?"

What I am claiming is that if we are given a tautological existential assertion like "Particulars exist" or "God exists", the existential question is not settled. Just as the tautology, "Fictitious objects do not exist", leaves open the question whether there are fictitious objects, so the tautology, "God exists", leaves open the question whether there is a God. But what is this further question? How paradoxical it seems to deny that "once one has grasped Anselm's proof of the necessary existence of a being a greater than which cannot be conceived, no question remains as to whether it exists or not".<sup>1</sup>

It is tempting to try to resolve the paradox in accordance with Aristotle's principle that "there are several senses in which a thing may be said to 'be'". Then to say that fictitious objects do not exist would be to say that fictitious objects lacked, say, spatio-temporal existence, whereas to say that there are fictitious objects would be to say that they had some other kind of existence—hence no contradiction, since in a sense fictitious objects exist and in a sense they do not. But appeals to the systematic ambiguity of "exists" will not work in all cases, for we may deny that there are A's in precisely the same sense of "be" that we claim tautologically that A's exist. For example, it will be tautologically true that particulars exist in precisely the same sense of "exist", say, temporal existence, that it might be true that there are no particulars.

A more promising line of argument consists in showing that a tautological existential claim is quite different from a non-tautological existential claim. How are we to explain the difference? Suppose we say that a tautological existential assertion consists in attributing to the subject a special property, the property of necessary existence. We could explain this property by saying, *à la* Malcolm, that a being which has this property is such that it is senseless to speak of its non-existence or of its coming into existence or going out of existence or of the existence of anything else as a condition of its existence (pp. 44-59). Now this account will not do. First, the attempt to explain the necessity of the statement by postulating a special property commits us to an infinite regress of properties, for presumably this special property might not be one which a being just happens to have but one which it necessarily has and which it is senseless to speak of its

<sup>1</sup> Malcolm, *op cit* p. 52

not having, and thus by similar reasoning we are led to necessary necessary-existence, etc. And it is most unclear what these properties could be or how we could distinguish them. But, secondly, it is not clear what this property of necessary existence is, if this is any more than a way of saying that the existential proposition is necessary. Am I making anything clearer when I say that squares, which are necessarily four-sided, have the special property of *necessary four-sidedness*? A defining property is not a special kind of property. So the tautological character of the existential assertions I have been discussing cannot be explained by postulating a special predicate, necessary existence. Their tautological character arises from nothing but the definition we have stipulated for the subject term. But then we are still left with our puzzle: how is it possible to say that A's necessarily exist but there may be no A's?

I wish to consider one further attempt to remove the paradox, one suggested by some remarks by Carnap on a somewhat different issue:

If someone wishes to speak in his language about a new kind of entities, he has to introduce a system of new ways of speaking, subject to new rules, we shall call this procedure the construction of a *framework* for the new entities in question. And now we must distinguish two kinds of questions of existence: first, questions . . . *within the framework*; we call them *internal questions*, and second, questions concerning the existence or reality of the *framework itself*, called *external questions*.<sup>1</sup>

Carnap goes on to explain that "external questions" concern nothing but "whether or not to accept and use the forms of expression for the framework in question", a purely practical question to be answered in terms of expediency and fruitfulness (p. 23). To apply this distinction to our paradox, given the basic definitions and rules of a particular religious language it will be a necessary statement that God exists (although not, perhaps, a necessary statement that the Devil exists), but we may ask the further question, Is there a God? meaning, Is this language a useful one?

Now waiving objections we may have about the vagueness of talking of the "fruitfulness" of a set of expressions, it still remains the case that "Are there any A's" will not always be identical with asking if the language is fruitful, for it may be very fruitful to talk of perfect pendulums, frictionless pulleys, the

<sup>1</sup> R. Carnap "Empiricism, Semantics, and Ontology". *Revue Internationale de Philosophie*, §11, January 1950, pp. 21-22.

ideal society, Euclidean points, and the economic man, even if it is perfectly clear that there do not exist such things. So the question whether there are such things cannot be identical with the question whether it is in some sense fruitful to use expressions which refer to them; a fiction or *façon de parler* may turn out to be useful and fruitful.

What lies at the heart of the puzzle about the Ontological Argument is the fact that our concepts have two quite different aspects, marked by the familiar philosophical distinction of intension and extension. A word like "horse" has a particular meaning and is logically connected with other words like "animal"; its corresponding concept, the concept of a horse, has a particular content and is connected with other concepts like the concept of an animal. It is this intensional feature of words and their corresponding concepts which makes certain assertions like "A horse is an animal" tautological. But words and concepts are also applicable to things. It turns out to be the case that there have existed, do now exist, and will exist entities such that it is true of each of them that it is a horse, true of each of them that the concept of a horse applies to it. And this fact we may express by saying that the word, "horse" or the concept of a horse has extension. In making assertions about the extension of a concept there are typical forms of expression which we use: "... exist", "... are non-existent", "There are ...", "There are no ...", "... are plentiful", "... are scarce", "... are extinct", "... are mythological", "... are found in Africa", etc. That such expressions are typically used in assertions about the extension (or lack thereof) of particular concepts is what is correctly brought out in the slogan, "'Exists' is not a predicate". But the typical use is not the only use. Since any statement, with suitable definition, can be true by virtue of the meanings of the terms, sentences with existential expressions can be used to express tautological statements. The very same sentence which is typically used to make a claim about the extension of the concept may instead be used to make a claim about the intension of the concept. We cannot tell by the form of the expression how the expression is being used. "Particulars exist", when asserted tautologically, is used to make a claim about the *meaning* of the word, "particulars", and therefore cannot be used to make a claim about the extension of the term. Similarly, if someone uses the sentence, "God exists", tautologically, he tells us only that being an existent is a logical requirement for being God. If, on the other hand, someone asserts, "God exists", non-tautologically, then he claims that the term, "God", has extension, applies to some existent.

In the case of the Ontological Argument the only valid conclusion is an intensional statement about the meaning of the concept of God. *A fortiori* the conclusion cannot be about whether anything exists to which the concept applies. The *prima facie* plausibility of the Argument comes from the use of a sentence intensionally when the typical use of that sentence is extensional. In this way it conceals the illicit move from an intensional to an extensional statement.

It looked as if the familiar distinction between intension and extension, stood in danger of breaking down in the case of existential tautologies. But we have seen that this is not the case. For even when we have an existential tautology like "Particulars exist" or "God exists", it still remains an open question whether the concept of particulars or the concept of God has application, applies to any existent. What is settled at one level is not settled at another level. It is important to see that we can go on to settle the question at the other level, too, for we can *make it a priori* true that the concept has application. For example, let the expression, "the concept of God", mean "a concept which has application and applies to a being such that...". Then by definition the concept of God has application; the statement, "The concept of God has application", is now a tautology, given the definition. But nothing is gained by such a manoeuvre. We have given the expression, "the concept of God", a meaning; we have framed a concept, namely the concept of the concept of God, and this concept makes certain statements tautologically true. Yet we can still raise the extensional question, Does this concept refer to any existent? At this level the extensional question would be whether there actually is a concept of God such that this concept has extension, and there is such a concept only if there actually is a God. So making the condition of having application or extension a necessary condition for being a concept of God still leaves open the question, concerning *that* concept, whether it has extension. Nothing has been settled except the meaning of a certain expression.

Why is it that extensional assertions cannot be tautological? Because they do not merely tell us what the requirements are for being an *A* but, starting with these requirements, tell us whether anything meets these requirements. Even if it is a conceptual requirement that the thing exist in order to be an instance of the concept, that in no way settles whether the requirement is met. And if we *make it* a tautology that the requirement is met, by framing a concept of a concept, then we are left with the open question whether the newly framed concept has extension. That



is what is true in the thesis that no "existential" proposition can be analytic. But we must remember that an "existential" proposition can turn out to be an intensional proposition, and therefore tautological.

Since much of what I have claimed depends upon the legitimacy of the intension-extension distinction, I wish to consider, finally, two threats to this distinction. The first concerns the so-called *intensional object*. When I conceive of an object, think about it, describe it, make a painting of it, long for it, look for it, and expect to find it, it may nevertheless be the case that the object does not exist, that the concept has no extension. But it is tempting to say that there must be something such that I conceive of it, think about it, describe it, etc., tempting to say that the object in some sense exists. And thus it is tempting to say that the mere fact that there is a concept of some object entails that the object in some sense exists. Well, even if one says that, it is obviously not the sense in which the religious usually wish to say that God exists nor the sense in which the atheist wishes to deny that God exists. They disagree about whether anything answers to that concept of an object, not about whether that concept is a concept of an object.

A second, and more troublesome, threat to the intension-extension distinction arises when we try to apply the distinction to certain concepts. We seem quite clear that the concept of a horse does have extension and that the concept of a unicorn does not have extension, and that these are contingent facts. But now suppose we ask whether the concept of a number has extension. If we hold that the concept ultimately has as its extension things in the world, then it still remains a contingent fact that the concept has extension. But suppose we are inclined to say that the concept has extension simply because, as we all know, there are (infinitely) many numbers. Surely it is not a contingent fact that there are (infinitely) many numbers. So if this fact leads us to say that the concept of a number has extension, then it will be a necessary proposition that the concept of a number has extension and, given the concept of a number, we can say *a priori* that the concept applies to (infinitely) many things.

What makes this case puzzling is that we have no idea what would count as establishing that the concept of a number has extension or that it does not have extension. We can investigate whether the concept of a number is a legitimate one, clear and self-consistent, we can note its logical connection with other mathematical concepts; and we can frame propositions which state these connections, even propositions like "There exists a

number which is even and prime". But what would count as showing that the concept, over and above its intensional content, has extension as well? Where would one look for traces, signs, evidences, intimations, or testimonies of the existence of numbers? Would we not say of someone who did think such a search sensible that he had misconceived the nature of numbers? Nothing would count as showing that the concept of numbers had extension over and above its intensional content, and this is to say that the notion of extension does not apply here. The most that could be said is that numbers are *intensional* objects.

The same thing must be said for the existence of God. The most that the Ontological Argument establishes is the intensional object, God, even if this intensional object has the attribute of existence as an intensional feature. To establish that the concept of God has extension requires adducing some additional argument to show that over and above its intensional features, over and above the content of the concept (or the meaning of the word, "God"), the concept of God has extension as well. This additional argument will of necessity have to be an *a posteriori* argument to the effect that certain evidences make it reasonable to think that some actual existent answers to the concept. We are thus led to the result that the Ontological Argument of itself alone cannot show the existence of God, in the sense in which the concept is shown to have extension. And this is just as the religious wish it to be. They do not conceive of God as something whose being expresses itself entirely in the concepts and propositions of a language game. They conceive of Him as something which has effects on the world and can in some way be experienced. Here is a crucial respect in which His status is meant to be different from that of the numbers. The concept of God is a concept which *might* have extension. But some further argument is required to show whether it does or not.

Concepts are like nets. What they catch depends in part upon how we construct them and in part upon what is outside the net. Suppose I produce a net for catching fish one-millionth of an inch long. Of such a net we are entitled to say, "This net catches fish one-millionth of an inch long", and what shows that this statement is true is nothing but the construction of the net. Does the net catch anything? It catches fish one-millionth of an inch long. Still, a question remains. Shall we ever find such fish in our net? For those who hunger for such fish, the existence of the net does not in any way show that what they hunger for shall be given unto them.

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### III.—RATIONAL BEHAVIOUR AND PSYCHOANALYTIC EXPLANATION

BY PETER ALEXANDER

#### I

It is often said that psychoanalysis has drawn our attention to the irrational springs of human behaviour. Recently, however, I have heard it said<sup>1</sup> that, on the contrary, psychoanalysis has revealed that our behaviour is more rational than we usually suppose it to be. The neurotic, according to this view, is radically misinformed but on the information he has he behaves rationally and if he discovers, or is supplied with, information he lacks he does the rational thing and alters his behaviour accordingly. This is a tempting and persuasive view but, it seems to me, a misleading one which calls for a detailed examination. I shall first show why the view appears to have some force, then discuss the notion of rational behaviour and finally raise some objections to the view.

In *The Psychopathology of Everyday Life*, and many other places, Freud argued that many pieces of apparently accidental, haphazard or purposeless behaviour could be explained in terms of unconscious wishes or purposes. I give three of his examples which will be convenient for testing the view in question.

(1) A woman patient always read "storks" instead of "stocks" and did not know why she did so. Freud explained this by discovering that she had no children but badly wanted them.

(2) Freud found that on a sheet of notes about his daily engagements he had himself written the correct date, September 20th, and, under it in brackets, October 20th. He could not remember doing this and was, at first, completely mystified by it. He finally explained it in terms of an unconscious wish. He had just returned from a holiday, feeling fit and ready for work, but he had very few patients. He had, however, a letter from a patient saying that she would come to see him on October 20th. He concluded that his mistake sprang from a wish that the intervening month had passed. He accounts for his easy discovery of the explanation by the fact that the "disturbing thought" was not unpleasant.

(3) A young woman with a jealous husband danced a can-can at a party. Everyone was full of praise except the husband who accused her of behaving like a harlot ("once again"). The next

<sup>1</sup>By Mr. J. W. N. Watkins and others, in discussion and conversation.

day she went driving in a carriage, jumped from the carriage because, she said, she was afraid that the horses were going to bolt, and broke her leg. Her unconscious purpose, Freud says, was to punish herself for her forwardness and make it impossible for her to dance the can-can in the immediate future. (There is a good deal more supporting evidence for Freud's explanation.)

The idea that apparently purposeless and innocent slips of tongue and pen can be explained in terms of unconscious wishes and purposes is connected with the Freudian theory of the generation and cure of neuroses. Even the simple examples I have quoted involve minor neurotic symptoms but in the fully-blown neuroses greater stress is laid on the unconsciousness of the purposes and wishes and the difficulty of discovering them. The general theory is, briefly, that a person who has desires of which he is ashamed or frightened protects himself from them by "pushing them into the unconscious", forgetting them beyond all normal power of recall, and substituting for the behaviour which would satisfy them some more innocent behaviour which, however, is mysterious both to himself and to the ordinary observer. Such behaviour is often referred to as a "symbolic" satisfying of the forbidden desire and is also regarded as inflicting a punishment for the repressed wish. It is mysterious because it does not appear to serve any purpose and no adequate reasons for it are obvious.

The view I am considering stresses the central assertion of the Freudian theory that it is possible to explain much of our behaviour in terms of unconscious wishes, purposes, and so on. The behaviour which we usually call "irrational" can be shown to be based on reasons which can be unearthed by psychoanalysis. The examples I have given show typical, though elementary, explanations of this sort. At first sight it seems plausible to say that since reasons can be given for pieces of behaviour we usually call "irrational", even this behaviour is, after all, rational, but at the unconscious level. It is a short step to the conclusion that Freud has shown irrational behaviour to be "really" rational and that we are, therefore, more rational than we usually suppose. I am inclined to think that the step, though short, is in the wrong direction.

The view is connected with a general account of human behaviour according to which the only way to explain an *action*, as distinct from a mere physical movement, is by showing that it was the rational thing to do. Attempts to explain actions in terms of physical causes are, at best, no more than explanations of physical movements. attempts to explain them in terms of

forgetfulness or clumsiness, or some other such factor, amount to admissions that the actions cannot be explained or to assertions that they were not actions at all. I do not intend to discuss, here, this general view but only its application to psychoanalytic explanation.

It is important, here, to mention the distinction, to which I shall return, between rational *beliefs* and rational *behaviour*. It may be rational or irrational to hold a given belief but given that I hold it, I may act rationally or irrationally on its basis. If I firmly believe, falsely and on insufficient evidence, that my neighbour is planning to poison me, that is irrational. But it is rational, *given* my firm belief, to avoid drinking tea in his house and to instruct my wife not to leave him alone in our kitchen.

The view in question is that the neurotic behaves rationally in this sense. He unconsciously holds a number of irrational beliefs but, given these beliefs, he behaves rationally. Psychoanalytic explanation, it is argued, involves discovering these beliefs. The theory of cure is that if the neurotic discovers, or is provided with, the information he lacks he will abandon these beliefs because he will see that they are irrational. He will, in consequence, stop behaving in a neurotic way. It is sometimes even argued that to dispel these beliefs, and to bring about a cure, it is *sufficient* to bring them to light, when their absurdity will be evident to the patient.

I am concerned chiefly with the alleged rationality of the behaviour rather than the unquestioned irrationality of the beliefs, and with psychoanalytic explanation rather than cure. It is necessary for my purpose to examine the distinction between rational and irrational behaviour and to say something about explaining behaviour. These topics are the subjects of my next two sections.

## II. *Rational and Irrational*

It may well be that there are various senses in which the word "rational" is used in connection with behaviour. I shall discuss here what appears to me to be a central and important sense in which it is commonly so used and try to show that, at least in this sense, the view I am considering is mistaken or misleading.

There is a difference between saying that a given piece of behaviour would be rational in a given situation and saying that *A's* behaviour in that situation was rational. What makes a given piece of behaviour rational in a given situation is that there are good reasons for behaving thus; what makes *A's* behaviour

rational is that he behaved in the way he did *for* those good reasons. The good reasons were *his* reasons. He *had* those reasons for behaving thus. I shall mainly be working towards a closer analysis of what it is for a given person's behaviour to be rational. I shall not, I am afraid, arrive at a complete analysis of rational behaviour in this sense. but it will be sufficient for my purpose if I can correctly establish certain of its characteristics.

As a first approximation I might suggest that *A* behaved rationally if he behaved thus for a reason. But this clearly will not do because we should not say this if *A*'s reason was a bad one. I may do something for a reason without its being the reasonable thing to do or, *a fortiori*, rational. This is implicit in the fact that I can correctly say that *y* was *my* reason for doing *x* while admitting that *y* is not *a* reason or *a good* reason for doing *x*. I may, for instance, have done *x* because I thought it would achieve *y* but now I see that it could not possibly have done so.

Thus we must add to the first suggestion that the reason was a *good* reason. But this is still inadequate since any reason for doing *x* cannot be a very bad reason and a reason for doing *x* may be good without being sufficient. One good reason for doing *x* may be outweighed by several good reasons against doing *x* or it may not *by itself* (*i.e.* without other good reasons) constitute a sufficient reason for doing *x*. In order for *x* to be rational I must have sufficient reason for doing *x*, that is a reason or collection of reasons which is strong enough to stand even after weighing the important reasons for and against doing *x*. There may be a reason, a good reason or good reasons for doing *x* without there being sufficient reason. So we have at least one further necessary modification of the original suggestion. Let me say that *a piece of behaviour was rational if it was done for reasons which constitute a sufficient reason*.

I must now consider what it is for something to be a reason and to be a sufficient reason for certain behaviour. If I say that *y* is a reason for doing *x* I imply that *x* will achieve, or help to achieve, *y*, that the behaviour is somehow appropriate to what it is intended to achieve; if I say that *y* is not a reason, or is a bad reason, I imply that *x* will not achieve or help to achieve, *y*, or that it is unlikely to do so or that it will not do so as economically as some other behaviour would or that it will also produce other undesired consequences. If *x* is very unlikely to achieve *y* or could not possibly do so we could not correctly say "*y* is a reason for doing *x*" although I could still say "My reason for doing *x* was *y*". If I say that *y* (which now may be complex) is sufficient reason for doing *x* I mean that *x* is likely to achieve *y* and

that  $y$  is valued above all other things which may be brought about by doing  $x$  but which I do not want to bring about or which I want *not* to bring about

There are some difficult cases which do not at first sight seem to fit into this account of reasons. It might be said, for example, that when I thank someone for a present I do not aim to achieve anything and even that it is not correct to say that I had any reason. I think that there are perhaps cases in which thanking someone for a gift is just a spontaneous gesture which would appear to be too calculated if we said it was done for a reason.<sup>1</sup> I doubt if this behaviour would be said to be either rational or irrational, but I am very unsure about this. Of other cases it might be said that the fact that someone gave me a present was a reason, and perhaps sufficient reason for thanking him, and that no mention need be made of my wishing to achieve anything. But in such a case I think we can always say that a reason for thanking him was that I wanted to show him that I was pleased, or grateful (or that I wanted to conform to convention or . . .). The question "Why did you thank him?" can be answered by "Because he gave me a present", but it can then be asked again in expectation of some such answer as "because I wanted to show my gratitude". I am inclined to think that whenever it is appropriate to ask for a reason for a piece of behaviour, it is possible to give a reason in terms of someone's wanting or intending to achieve something, even if in certain cases what it is intended to achieve is consistency or appropriateness or a state of mind in someone else.

I can now expand my account of rational behaviour. A piece of behaviour was rational if it was done for reasons which constitute a sufficient reason, that is, if it was likely to achieve what was intended and unlikely to lead to other consequences whose undesirability outweighs the desirability of what it was intended to achieve. It was irrational if it was not done for reasons which constitute a sufficient reason, that is, if it was unlikely to achieve what was intended, or less likely to achieve it than some other piece of behaviour, or was likely to lead to other consequences whose undesirability outweighs the desirability of what it was intended to achieve.

There is, of course, a scale of rationality between the most rational and the most irrational. Clearly it is irrational to do something which cannot possibly achieve what is intended and clearly it is rational to do something which is very likely to achieve it without any unwanted side-effects. In between we

<sup>1</sup> My thanks are due to Paul Ziff for convincing me of this.

often have to contrast alternative ways of behaving between which it is not easy to decide. In a given situation, other things being equal, it is rational to behave in a way which is more likely to achieve what is intended, less rational or irrational to behave in a way which is less likely or unlikely to achieve it.

There may be a sufficient reason for behaving in a certain way in a given situation but if this is not my reason for behaving in that way then I cannot be said to have behaved rationally, by reference to *that* reason. It is therefore necessary to be clear about what it means to act *for* a reason or with a reason in mind, to *have* a reason or to say that a given reason was *my* reason. An inarticulate person may sometimes be said to behave in a certain way for a reason, and even for a good or sufficient reason even though he is unable to say what this reason was. Habitual actions and those performed during the exercise of a craft may be done for good reasons and constitute rational behaviour although the agent was not conscious of the reasons before, during or after the actions. I may have a reason in mind without attending to it. However, I think that it is a necessary condition of my acting for a reason that I should be able to become aware of my reason if I think about my behaviour, although I need not be able to state it. To say that a given reason was *my* reason is to imply that, if I think of that reason or someone suggests it to me I can recognize it as *my* reason, or one of my reasons, for that behaviour (or as having influenced my behaviour). I doubt if it is correct to say "I acted for a reason" and at the same time to confess that, however hard I think about it, I cannot discover the reason and that, however many possible reasons are put to me, I cannot recognize any of them as *my* reason. If this is true of having a reason it is true of having a good or a sufficient reason. Having a reason, at least in this sense, is a necessary condition of behaving rationally.

It would be as well to mention here a consideration which will become important later. There is a perfectly ordinary, everyday sense of "unconscious" in which while I am not thinking of something I may be said to be unconscious of it. I am at the moment unconscious of what I had for breakfast this morning because I am not thinking of it or trying to recall it, but if I did try I could probably recall it. Similarly I may have a reason for behaving in a certain way and yet be unconscious of my reason at any given time. This is to be distinguished from the technical sense in which "unconscious" is used in psychoanalysis, according to which what is unconscious is beyond all our normal powers of recall.



If what I have said so far is correct, both rational behaviour and irrational behaviour are such that they could have been consciously planned even if they were not. My account allows, as I think we normally would allow, that some habits and learned skills may be said to be rational. Irrational behaviour resembles rational behaviour in the sense that it is the sort of behaviour which it is possible to give reasons for and against.

I have used the words "intention" and "intend" because I wish to exclude from both categories such things as reflexes and sheer accidents. For these, if we are to call them "behaviour", we need a third category which may be called "non-rational" and includes any behaviour of which it does not make sense to say either that it was or was not done for a reason. Non-rational behaviour could not be intended; for example, fainting or jumping when startled (*real* fainting and *real* jumping), or sheer accidents involving unforeseeable events, like unavoidably running over someone who runs out in front of a car. I cannot intend to faint or run a person over unavoidably.

It might be thought to be an objection that doubt can be cast upon the rationality of behaviour which tends to achieve what is desired, by questioning the rationality of seeking to achieve the particular thing desired. Behaviour which, in a narrow context, looks rational may be regarded as irrational in a wider context. Given that I wish to commit suicide it is rational to choose the gas oven rather than the electric oven but there may be more rational ways out of my difficulties than self-destruction. This is not, however, an objection to my view since suicide would be judged to be rational or irrational in the light of yet other things desired and the criteria I have outlined would simply be applied in the wider context.

Similarly, it may be rational to behave in a particular way given the beliefs I have about certain matters of fact but these beliefs may be irrational because they conflict with the evidence I have or evidence I could easily get or are based on careless or mistaken reasoning. But if I believe that *w* is the case and that, in consequence, I ought to aim to produce *y*, then behaviour that tends to produce *y* may be rational, within the narrower context, no matter how wrong is my belief about *w*. The rationality of behaviour is relative to context.

This bears obliquely on my reason for *not* saying that behaviour which is rational must achieve what is intended *in the way the agent thinks it does*. Suppose that we have discovered empirically that railway lines buckle in hot weather when gaps are not left between them, although we know nothing about the relation

between heat and expansion. Then it is rational to leave gaps between them so that they do not buckle, even if we think we do this in order to appeal to the aesthetic senses of the gods so that they will refrain from bending the rails and even if, when we have learnt about heat and expansion, we no longer think it is rational to leave gaps *because* the gods prefer it.

### III. *Explaining Behaviour and Justifying Behaviour*

It is easy to mistake a justification for a piece of behaviour for an explanation of it and I suspect that such a mistake is involved in the view I am considering. The mistake may arise in this way. A person behaves in an unusual way and, without consulting him, we ask ourselves why he behaved as he did. That is, we look for reasons for his behaviour. If we are able to find a sufficient reason we may think we have explained his behaviour, but the most we can be sure we have done is to justify behaviour of this sort in this kind of situation. We have not explained *his* behaviour until we have discovered what *his* reasons were. We have discovered something that would explain it but not necessarily what does explain it. We may justify such behaviour by showing that it achieved something of which *we* approve but we can explain it only by showing what *he* intended to achieve.

Suppose, for example, that for a month our friend eats and drinks nothing but milk. We cast about for explanations and decide, since he has shown some interest in nutritional problems, that he wished to show that milk is a complete food for an adult. (There are, of course, several other reasonable conclusions we might reach.) This would be a good reason for his behaviour and sufficient, we think, to justify it. But if we are ignorant of our friend's reasons we cannot say that this was his reason and although we have justified such behaviour to ourselves we have not explained it. We have shown how behaviour of this sort might be shown to be rational but we have not shown that he behaved rationally. The most we can say is that if this *was* his reason it would both explain and justify his behaviour.

He may now deny that we have found his reason. There are two possibilities. First, he might give another good or sufficient reason, which would both explain and justify his behaviour. Second, he might give another reason which was a bad one. This would explain his behaviour but not justify it or show it to be rational, just because it was a bad reason. Thus to explain a piece of behaviour is not just to show that it was a rational thing to do and to show that it was a rational thing to do is not

necessarily to explain it. To explain a piece of behaviour we have to show what reasons the agent had to show that he behaved rationally we have also to show that these reasons were sufficient. We may therefore justify such behaviour as *A*'s without either explaining it or showing that he behaved rationally and we may explain his behaviour without showing that he behaved rationally.

There is an ambiguity about the word "reason" which I should perhaps get out of the way. I refer to the fact that we occasionally use "reason" in the sense of "cause". There is sometimes a difference between saying "My reason for doing *x* was *y*" and "The reason why I did *x* was *y*". The second form may be used to indicate causes as well as reasons in the strict sense I have been using. I can correctly say "The reason why I fainted was that insufficient blood was getting to my brain". Thus a statement of the form "The reason why I did *x* was *y*" may be used either to reinforce the assertion that my behaviour was rational or for the different purpose of giving a causal account of what I did. It would be odd to say "My reason for fainting was that insufficient blood was getting to my brain", because it suggests that on discovering the physiological fact I realized that the appropriate thing to do, finding no reasons against, was to faint, so I fainted. I can, however, say either "The reason why I bought a thermometer was that I *wanted* to measure my temperature" or "My reason for buying a thermometer was that I wanted to measure my temperature". The need to include the word "wanted", or some such word, in the description of the reason, in the strict sense, is important.

#### IV. Criticisms

When we are considering behaviour in the ordinary way, and not theorizing about it in the manner of psychoanalysis, we find two sorts of behaviour which are conveniently labelled "rational" and "irrational". It is helpful, or sometimes essential, to contrast these with one another and with the sort of behaviour which can be called "non-rational". But psychoanalytic theory implies that all the behaviour which we call "irrational" can, at least in principle, be explained in terms of unconscious purposes, and so on. If we go on to say that this shows such behaviour to be rational we blur these distinctions. There would be nothing wrong with this if it were accompanied by arguments showing these distinctions to be inaccurate or unnecessary, but such arguments do not seem to be forthcoming. As we shall see, nothing

of this sort follows from psychoanalytic theory, so it is difficult to see how such arguments could be upheld.

Here it is sufficient to note that if we say that our irrational behaviour has been shown to be "really" rational we allow "rational" to be used only in contrast to "non-rational". It is not very instructive merely to distinguish our faintings and unavoidable accidents from all the rest of our behaviour, and to make no further distinctions. In making moral judgments, ascribing responsibility, assessing intelligence, cleverness, reliability and a host of other activities it is essential to make finer distinctions. Moreover, the problems in which psychoanalysis originated depend upon this distinction. The meaning of "rational" in such contexts involves the contrast with both "irrational" and "non-rational". If this is so, to assert that our irrational behaviour has been shown to be rational is to use "rational" in a new sense while pretending to use it in the familiar sense, for it leaves us no behaviour which can be said to be, either consciously or unconsciously, irrational.

This frustrates any attempt to make the view more acceptable by saying "Freud has shown us that behaviour which is consciously irrational is unconsciously rational". This is not to say that he has shown us that we behave more rationally than we usually suppose, for it is not to say anything about the rationality about which we usually suppose. We usually, I claim, suppose people to be rational to the extent to which their behaviour fits the criterion I have outlined, or something like it. We do not, without benefit of Freud, entertain ideas about unconscious rationality. This formulation does, indeed, leave the distinction between consciously rational and consciously irrational behaviour but it weakens it by suggesting that unconscious rationality differs from conscious rationality only in being unconscious. This is misleading because Freud has not left us any behaviour which is unconsciously irrational with which to contrast that which is unconsciously rational. "Rational", I suggest, is being used in such different senses at the conscious and unconscious levels that we must be very careful how we draw an analogy between them.

We might, in an attempt to save the view, retreat still further and claim that Freud has shown our irrational behaviour to be *more like* our rational behaviour than we supposed, since whereas we formerly thought that we could not give good reasons for it we now find that we can. This is to draw an analogy between psychoanalytic explanations and certain ordinary explanations of behaviour in terms of reasons for it. The value of this analogy depends on the extent to which the good reasons for

"irrational" behaviour are like the good reasons for rational behaviour. A more detailed examination will show that the reasons adduced in psychoanalytic explanations are very unlike what we would normally regard as good reasons and consequently that the behaviour in question is shown to be rational only in a new and unfamiliar sense.

I shall come to this more detailed examination by recapitulating three points I made earlier and applying them to psychoanalytic explanations. In ordinary circumstances (1) we may explain a piece of behaviour by showing that it was done for a reason or with a reason in mind and showing what that reason was; (2) to explain a piece of behaviour is not necessarily to show that it was rational; (3) to show that somebody's behaviour was rational it is necessary to show (a) that the agent had reasons and (b) that the reasons were sufficient reasons.

Thus the fact that a psychoanalytic explanation can be given for a piece of otherwise unexplained behaviour does not show that behaviour to be rational unless (a) the reasons given were the reasons for which the agent behaved as he did and (b) they were sufficient reasons. It seems to me that there is some doubt about the possibility of satisfying the first condition and that the very nature of the typical psychoanalytic explanation makes impossible the satisfying of the second. This is not to claim that psychoanalytic explanations are not explanations or are useless but only that they are of a very different kind from everyday explanations of rational behaviour. I shall discuss these two conditions separately.

(a) I have argued that when we say that I did something for a reason or with a reason in mind we imply that I must be able to discover that reason or recognize it when it is suggested to me as having influenced my behaviour. It must, at least in this sense, be *my* reason. I may recognize a suggested reason as a good reason without recognizing it as *my* reason and I may have to admit, if I am being honest, that *my* reason is not to be found among the good reasons.

The sense in which we normally allow that a person may be unconscious of his reasons for acting demands that he should be able to discover them by everyday methods of self-questioning or at least to recognize his reasons when they are suggested by others using similar everyday methods. But the special sense in which "unconscious" is used in psychoanalysis may make all the difference. since a person's unconscious reason, in this sense, is *ex hypothesi* beyond his power to discover without the assistance of special techniques and, usually, another person trained in these

special techniques, working on the basis of a special theory. Neurotic behaviour is mystifying to both the neurotic and the ordinary observer. The discussions and other transactions necessary to dissolve the mystery are very unlike the discussions we normally go through in finding explanations for normal behaviour.

The nature of the techniques employed is very important in the present context. Are they such that the patient can be said, when he has accepted the explanation, to have *recognized the reasons* as those which in fact influenced his behaviour? This I doubt. Case histories show, again and again, that the patient meets the suggested reason as a stranger, failing to recognize it or resisting it violently, until a good deal of preparatory work, including the expounding of bits of theory, has been done by the psychoanalyst. Is this preparatory work correctly described as showing the patient that these were in fact his reasons rather than showing him that, given the theory, these reasons are "good", i.e. fit the theory? Is his acceptance more correctly described as seeing what his reasons were or as seeing what his reasons *must have been*? ("must" not, of course implying *logical necessity*) Of course, his resistance to the reasons is explained by the theory. We are all familiar with our own inner struggles not to admit a disreputable reason for which we have acted but we are also familiar with our own reluctance to admit a disreputable reason which we are sure did not influence our action. Moreover, the importance of the transference situation suggests, very strongly, I think, that the processes involved are not just those associated with intellectual conviction.<sup>1</sup>

When the patient accepts or "discovers" unconscious reasons he may be saying "Now I see what my reasons must have been, though I did not suspect it" or he may be saying "Now I see what my reasons were but I had forgotten them". It is easy to suppose that the alleged reasons were not in fact effective reasons, did not in fact influence the behaviour, but rather that the behaviour can be interpreted *as if* they were and moreover that the cure can be achieved if this interpretation is accepted, whether or not it is correct. I am suggesting that the process may be more like the normal justifying of an action than the normal explaining of an action even though what issues from it is not very like a justification for the action. There is, of course, the connected problem of how it is possible to discover that we are remembering reasons

<sup>1</sup> Patrick Mahoney in *On the Psychology of the Neurotic* (New York, 1961) writes: "The central problem concerning the curative relationship is the problem of the illness effect cure."

we had forgotten, especially when the forgetting is of the Freudian type.

(b) Compared with the ordinary acceptance of reasons for behaviour, the acceptance of the psychoanalytic kind of reason for behaviour which before looked irrational is odd in another way, because we can never have been conscious of having *such* reasons for *such* behaviour. The theory holds that unconscious reasons are effective only because they are repressed and so unconscious in the technical sense. In the ordinary way, before we say that a piece of behaviour was rational we demand that a sufficient reason be given for it. A sufficient reason is such that we can see that it could have been a reason for this particular behaviour and, I think, that we can conceive of ourselves as behaving thus with this reason in mind. We demand appropriateness.

I doubt if the typical Freudian reason can satisfy this condition. Such a reason is a reason for this behaviour only because it was unconscious. We can never know what it would be like to act thus with this reason in mind since it does not make sense to talk of acting with unconscious (in the technical sense) reasons in mind. The typical unconscious reasons are not the sorts of reasons which would lead to that sort of behaviour if we were conscious of them. The shocked reaction "Good gracious, is that why I did it? I should never have done it if I had known" is typical and says more than the speaker, and perhaps Freud, usually realizes. The whole point of the theory is that neurotics behave as they do because they fool themselves completely about certain things, but we cannot fool ourselves completely and be aware that we are fooling ourselves. Unconscious reasons are not just possible conscious reasons for the behaviour in question. They would not be regarded as reasons for it if they were conscious.

This does not mean simply that the patient would see the reasons as disreputable if they were conscious but that he would see them as inadequate. For example, suppose that my lungeing at lamp-posts with my umbrella is explained by referring to my Oedipus Complex. My alleged reasons for behaving as I do are:

(i) I feel that my father hates me because we are rivals for my mother's love and I therefore wish to kill my father so that I do not have to share her love,

(ii) I am ashamed of and feel guilty about this wish so I conceal it from myself,

(iii) it is still effective so I "satisfy" it without realizing that this is what I am doing by some substitute activity such as lungeing at lamp-posts,

(iv) I want to punish myself for having this wish.

Now if my wish to kill my father were conscious it would be obvious to me that it was not adequately satisfied by my lungeing at lamp-posts. If my wish to protect myself from my own guilt were conscious it would be obvious that such behaviour would not help. That is, these "reasons" can be reasons for this behaviour only if they are unconscious for they would not look like reasons if they were conscious. The fourth reason looks, at first sight, more convincing. By behaving in an odd way I upset my relations with society by leading people to treat me as odd and so punish myself for my guilty wishes. However, this is not a very effective way of punishing myself as I would no doubt see if I were conscious of my guilty wishes and my desire to punish myself for them. The reasons would not appear to be appropriate to the form of my behaviour and we might further question the belief that the mere having of certain wishes merits punishment. It is not satisfactory, moreover, to regard all these factors taken together as constituting good reasons in the ordinary sense, even if they were all conscious the behaviour would not appear to be appropriate.

It is true that if my father hates me this may be a sufficient reason for protecting myself against him, but is it a sufficient reason for the neurotic behaviour in which I indulge? Only, I suggest, in conjunction with the theory. The relation between the reasons and the behaviour is not such that we would normally say that this was a sufficient reason for this behaviour. The relation needs explaining in terms of more theory. These considerations suggest that "reason", "good reason" and "sufficient reason" are used in this context in senses very different from their usual ones. In their ordinary senses, something which would not look like a sufficient reason for doing  $x$ , if we were conscious of it, is not a sufficient reason for doing  $x$ .

I can now return to the examples I quoted from Freud. In each of these, the behaviour does not seem to be related to the alleged reason in the way in which ordinary behaviour is related to ordinary good reasons for it. The woman who read "stocks" for "stocks" does not appear, by so doing, to have furthered either the end of obtaining children or of concealing from herself her own unhappiness and it is doubtful if she or others could have seen the behaviour as achieving anything except with the help of the Freudian theory.

Freud's own mistake about the date did not make the wished-for day come more quickly, nor bring him more patients, nor conceal from himself that he was short of patients. If he had been convinced that it was the correct date it might have done the last,



but he was not. His behaviour would have been perfectly rational if he had meant merely to remind himself that he had no important engagements until October 20th, but he neglects entirely this possibility. If he had not been so interested in explanations which fitted his theory might he not have thought that this was his reason? Moreover, it is easy to suppose that he did not recognize the reason he gives as his reason but inferred that it was his reason because it was the kind of reason he was looking for—and, indeed, he introduces the reason by saying “It was not difficult to explain . . .” (*i.e.* in terms of the theory)

The woman who jumped from the carriage could hardly have regarded her desire not to dance the can-can again or her desire to regain her husband's respect as good reasons for breaking her leg if she had been aware of them. She did, of course, punish herself for dancing the can-can and losing her husband's respect but I doubt if she would have found it the appropriate kind of punishment if she had been conscious of all this. It is true that the punishment does, in a sense, fit the crime but I doubt if many of us would regard our having danced the can-can as constituting sufficient reason for, and as being suitably punished by, the wilful breaking of our legs. Moreover, the husband's loss of respect here was due, Freud darkly hints, to more serious offences for which this is not a punishment with even this macabre kind of appropriateness.

In general, similar things can usually be said about Freud's explanations of neurotic symptoms. If I am said to do  $x$  for unconscious reason  $y$ , it is nearly always the case that  $y$  is not the sort of thing which we would normally consider a good reason for  $x$ . The theory is, of course, that  $y$  leads to  $x$  because it is repressed. The repression of the guilty thoughts is prior to the behaviour so that the behaviour cannot be thought of as fulfilling a wish to deceive oneself but only as fulfilling the repressed wish in spite of its being repressed or as punishing oneself for having the wish at all. The punishment itself seems always to be too severe, or not severe enough, or inappropriate in some other way.

It might be said that the reason given for the behaviour is the whole complex repressed-guilty-desires, that is, that the fact of repression is part of the reason. But the same arguments apply since if the neurotic could be conscious of having repressed his guilty desire this would still not look like a good reason for this behaviour. Moreover, this begins to look like an account in terms of causes rather than reasons, similar to “The reason why we hear scuffings is that there are mice in the cellarage”. It seems more accurate to say “The reason why he did this was that he had

repressed certain desires of which he felt guilty" than to say "His reason for doing this was that he *wanted* . .". I cannot discuss this here but wish merely to point out that there is a danger of confusing these two senses of reason in this situation. I have argued elsewhere<sup>1</sup> in favour of a causal interpretation of psychoanalytic explanations and cure but this would clearly be incompatible with the view I have been examining.

It seems to me that we can say that Freud has shown that it is possible to construct a theory on the basis of which irrational behaviour can be interpreted *as if* it were the outcome of given unconscious reasons. There is an apparent analogy between psychoanalytic explanations of irrational behaviour and ordinary explanations of rational behaviour. But the analogy can be pushed so little that it seems more of a hindrance than a help to use "explanation" and "reason" as if these words were used in the same senses in the two contexts. I have tried to show that unconscious reasons are very unlike conscious reasons and especially unlike what we normally call "good reasons" or "sufficient reasons" for behaviour. If we do call them "good reasons" it is clear that we use these words in an unusual sense and are therefore not entitled to go on to say that such explanations show our irrational behaviour to be really rational. This would be warranted if we added that "rational" was being used in a new sense but then the original statement would lose its point. I have no objection whatever to the use of words in new or unusual senses as long as it is clear that this is what we are doing and that it is in some way helpful or illuminating.

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<sup>1</sup> *Arist Soc Supp Vol 1955*

## IV.—IS THERE A PROBLEM ABOUT LOGICAL POSSIBILITY?

BY JOSÉ A. BENARDETE

ACCORDING to Hume, "it is an established maxim in metaphysics that *whatever the mind clearly conceives includes the idea of possible existence* or, in other words, that *nothing we imagine is absolutely impossible*".<sup>1</sup> Here at least is one "maxim in metaphysics" that our sceptical philosopher is prepared to accept as indeed "established". More than that. it is precisely owing to his reliance on that maxim that he is led to his bizarre conclusions. Our natural understanding assures us that it is utterly and completely impossible for a cow to jump over the moon—it cannot even jump over a barn. But it is Hume's conviction that there exists "an undeniable argument for its possibility". What is that "undeniable argument"? Simply this "We can at least conceive a change in the course of nature; which sufficiently proves that such a change is not absolutely impossible. To form a clear idea of anything is an undeniable argument for its possibility."<sup>2</sup> I propose to examine this alarming notion. How did Hume arrive at it? What evidence might be adduced in its support? Is there any such evidence? Hume admits that the underlying principle is not original with him. It has been "established . . . in metaphysics". Hume's contribution consists solely in exploiting that principle to a sceptical end.

The principle itself—'nothing we imagine is absolutely impossible'—is by no means self-evident to our pre-philosophic natural understanding. It requires proof. Far from being self-evident, it *seems* to be quite false. I can readily 'imagine' myself, in Hume's sense of 'imagine', lifting the Great Pyramid off the ground and high into the air, with my bare hands, unaided by any resource of modern technology. But no one in his right mind (philosophers excepted, and philosophy is a divine madness) supposes that my little fantasy in any way establishes the possibility of the exploit. We know—at least in our moments of common sense we fancy ourselves to know—that the exploit is utterly and *absolutely* impossible. We *call* the exploit impossible. It is a standard case, a paradigm case, of sheer impossibility, and in calling the thing impossible we do not deny that it can be 'imagined', *i.e.* pictured in the mind or drawn on paper. What we do deny is simply the inference that because it can be

<sup>1</sup> *Treatise*, Bk I, Part II, section 2 (Selby-Bigge, Ed., p. 32.) Hume's italics

<sup>2</sup> *Op cit* Bk I, Part III, section 6 (p. 89)

'imagined', therefore it must be 'possible'. With what right is that inference negotiated?

I have said that in common discourse we are not prepared to accept as valid the inference from 'imaginability' or 'conceivability' to 'possibility'. I must now qualify that statement. There is indeed a sense in which if something is acknowledged to be 'imaginable' or 'conceivable' it is also acknowledged to be 'possible'. That sense of 'imagine' or 'conceive' is, however, very different from that intended by Hume. The concepts of imagining and conceiving are by no means univocal in common discourse. We may say of them, with Aristotle, *λέγεται πολλαχῶς* or, with Wittgenstein, that they comprise a 'family of meanings'. Let me but ask, "Is it possible in the years ahead that war might break out over Berlin?" you will answer, "Yes, it is quite possible", or "I can very well imagine such a thing", or "It is certainly conceivable". These three alternative answers all mean the same thing. In the same way we should all insist that it is quite inconceivable that Paraguay will knock out the Soviet Union tomorrow in a great war—nothing of the sort can be imagined—it is absolutely impossible. Here, again, there is an equivalence of meaning in the three locutions. There is certainly no *inference*. If it is quite conceivable that war might break out over Berlin, then it is certainly possible. But there is no inference from the one to the other—from conceivability to possibility. So, too, if it is inconceivable and unimaginable that Paraguay should knock out the Soviet Union, it is also impossible. No evidence is here required to validate a leap from 'inconceivability' to 'impossibility'. The very same grounds that entitle us to say that the thing is inconceivable (they are not psychological grounds) also entitle us to say that it is impossible.

The sense of 'conceiving' or 'imagining' just noted—we may style it as the 'serious' sense—must be radically distinguished from a second sense in which these concepts are commonly employed. This second sense is the make-believe or storytelling sense, and it is identical with, or at least rather close to, the sense intended by Hume. I can certainly invite you to join me in an adventure of the imagination in which you are to imagine a cow jumping over the moon or even Paraguay defeating the Soviet Union with native foot soldiers fighting their way across the Ukraine. These things can be represented in a painting or narrated in a work of fiction. They are thus imaginable in the second sense: but they are quite inconceivable in the first sense, i.e. one cannot seriously imagine them actually to occur in the

real world. The two senses are so wildly different that as one reflects upon them one comes to feel that perhaps they have nothing in common but the name. Many have been inclined to think that it is the make-believe or pictorial or storytelling sense which is actually primary and that what I have styled the 'serious' sense is merely secondary and somehow derivative from it. Philosophers in general in their investigation of what it is to imagine or conceive of something, have chosen to concentrate on our pictorial and storytelling activities. They have felt that in the strict sense of the word—the concept being employed *ἀπλῶς* or *πρώτως* or *κυρίως*—we can certainly envisage or imagine or conceive of a cow jumping over the moon, and that it is only in some loose, colloquial sense—the concept being employed *πως* or *ἐπομένως* or *τρόπον τινά*—that it may be said that the thing is quite inconceivable or that nothing of the sort can be imagined. Construing the concept as a *πρός ἑν* equivocal, they have regarded the make-believe sense as privileged and primary.

I suggest that this way of looking at the matter is a mistake. It is rather the 'serious' sense which is primary and the make-believe or pictorial sense which is secondary. Serious things come first in life, play comes afterwards. The concept of make-believe is posterior to the concept of 'believe'. More directly to the point, perhaps, is the following consideration. It is only when the concept is employed in the 'serious' sense that a cognitive claim is made. To affirm that it is conceivable that war might break out over Berlin and, *a fortiori*, that it is possible; to affirm that it is inconceivable that Paraguay should knock out the Soviet Union and, *a fortiori*, that it is impossible—these certainly are cognitive claims. However, to entertain the nursery rhyme "Hey diddle diddle/The cat and the fiddle/The cow jumped over the moon", that is, merely to imagine a cow jumping over the moon, in a spirit of playfulness—this is not to advance any claim to knowledge. For it is not supposed that it is at all possible that a cow might really jump over the moon. Who could imagine such a preposterous thing? There is, then, one sense in which the thing is imaginable, the playful sense, and one sense in which it is not imaginable, the cognitive sense. The philosopher being one preoccupied with the problem of knowledge, he should be expected to concentrate upon the cognitive sense as privileged and primary.

Hume, among others, disappoints this expectation. More precisely, he satisfies the expectation but in a very peculiar way. He executes a surprising concept-reversal, there is a concept-shift. No longer is it the 'serious' sense which is cognitive,

cognition appertains now rather to the pictorial or make-believe sense. There is an exchange of roles here. Let me explain.

We are engaged in examining that "established maxim in metaphysics . . . that nothing we imagine is absolutely impossible." I have raised the question, is that maxim true? It has been seen that, viewed within the context of common discourse, the maxim is either true and trivial or significant and false, depending on the sense in which the word 'imagine' is employed. If the word is assigned its 'serious' non-Humean sense, the sense in which it is inconceivable and unimaginable that I might lift the Great Pyramid with my bare hands, then it is certainly true that if something is imaginable it is also possible. Imaginability in this sense entails possibility. This truth, however, is quite trivial. No evidence of any kind is required to validate it. To say that something is imaginable in this sense is simply another way of saying that it is possible. The terms are synonymous. On the other hand, if 'imagine' is assigned its pictorial or make-believe sense, then the maxim proves to be very significant indeed but, in the context of common discourse, it is quite false. I can fancy myself, in a moment of idle reverie, lifting the Great Pyramid unaided, but my reverie in no way establishes that the exploit is possible. If, then, the maxim can be read in either one of two ways, following the canons of common discourse, we may now ask in which of the two ways does Hume understand the maxim? The answer to this question is this: in neither way. Or, alternatively, we could answer the question in both ways. Hume certainly regards the maxim as both true and significant. If we may be allowed to distinguish the meaning of a word from its use, we may suggest that Hume effects a curious miscegenation. There are two questions here. What does Hume *mean* by the word 'imagine', and how does Hume *use* the word? The answer to the first question is that he means by 'imagine' what we ordinarily mean by 'imagine' when we employ it in its pictorial or make-believe sense. The answer to the second question is that he *uses* the word in the way we ordinarily use the word when we employ it in its 'serious' sense: he uses it as a warrant for possibility. Hume's concept is then very different from anything found in common discourse. Very much a *new* concept (it might even be styled a super-concept), it proves to be a violent birth born of a shotgun wedding between two concepts already familiar to us.

We have found that in common discourse 'possibility' attaches to 'imaginability' in one sense but it is quite absent from it in the other sense. Hume effects a reversal, and in transferring the concept of 'possibility' from the one sense where it normally

belongs to the other sense where it is normally out of place, he transfers the concept of cognition along with it. I am persuaded that it is inconceivable and impossible for me to lift the Great Pyramid, and I hold that conviction to be a matter of *knowledge*. Not so for Hume. My conviction is robbed of its cognitive status, it is demoted to the level of mere 'belief', mere 'habit'. Cognitively speaking, the exploit is viewed as really possible: it could actually happen. On what grounds? Because it can be imagined in the pictorial or make-believe sense. Which are no grounds at all, from the standpoint of the natural understanding.

Hume's concept-reversal issues in very radical consequences indeed. Prior to Hume it was held that the principle of causality, that "general maxim in philosophy that *whatever begins to exist must have a cause of existence*," was "one of those maxims which though they may be denied with the lips, 'tis impossible for men in their hearts really to doubt of".<sup>1</sup> Hume undertakes to explode that venerable principle by appealing to another metaphysical maxim which he never questions, namely that 'nothing we imagine is absolutely impossible'. He argues as follows.

'Twill be easy for us to *conceive* any object to be non-existent this moment and existent the next without conjoining to it the distinct idea of a cause or productive principle. The separation, therefore, of the idea of a cause from that of a beginning of existence, is *plainly possible for the imagination*, and consequently the *actual* separation of these objects is so far possible that it implies no contradiction or absurdity.<sup>2</sup>

How far possible is that? In the language of contemporary philosophical discourse, it is logically possible, which means scarcely more than that it is free of self-contradiction.<sup>3</sup> But is logical possibility a kind of possibility? Is counterfeit money a kind of money? Is a dead man a man? Can chess be played without the queen? Is it then *chess*?<sup>4</sup> Suppose that in common

<sup>1</sup> *Op cit* Bk I, Part III, section 3 (p. 79), Hume's italics.

<sup>2</sup> *Ibid* pp 79-80, italics mine

<sup>3</sup> Actually there is more to it than that. It is a fact that no idea is made of tan. There is no contradiction, in the strict formal logical sense, in asserting that some ideas are made of tan, yet that freedom from self-contradiction does not suffice to make the thing logically possible. What, then, is the defining criterion of logical possibility? 'Meaningfulness'? But what makes for 'meaningfulness'? Something is said to be logically possible or 'meaningful' if it can be imagined or conceived! So we are right back to our original problem: how does imaginability entail possibility?

<sup>4</sup> Aristotle's answer: λέγεται πολλαχῶς ἀπλῶς, 110, πως, yes. Wittgenstein's answer: Say what you please!

discourse some one were to suggest that there is, after all, a sense in which it is possible that I might lift the Great Pyramid, and upon being greeted with our astonishment, he were to reply that it is *logically* possible. We should protest, if we were simple men, that this was a kind of possibility that we had never heard of before. Has the philosopher discovered a new kind of possibility? Has he perhaps discovered that what hitherto has always been regarded as quite impossible is really possible after all? Or is logical possibility simply synonymous with freedom from contradiction? This last suggestion is too banal to be accepted as definitive. Freedom from contradiction is submitted as a *ground* establishing possibility. The one is held to entail the other. How is that inferential leap negotiated?

Hume's scepticism regarding the principle of causality is merely a special case of a more general scepticism—scepticism regarding all universal synthetic propositions, of which the principle of causality is but one instance. There is no doctrine in contemporary philosophy more firmly entrenched than that knowledge of universal synthetic connections is impossible or that universal synthetic propositions can never be conclusively verified. The following account of the doctrine has succeeded in commanding widespread assent.

We are told that [*i.e.* 'All men are mortal'] is not a doubtful hypothesis, as Hume maintained, but an instance of a necessary connection. . . . When a philosopher (Aristotle is the authority here—J. A. B.) says that the proposition 'All men are mortal' is an instance of a necessary connection, he does not intend to say that it is a tautology<sup>1</sup>. It is left to us to point out that this is all he can be saying. . . . [General propositions of law], as Hume saw, can never be necessary. However firmly we believe them, it is always conceivable that a future experience will lead us to abandon them.<sup>2</sup>

'Conceivable' in what sense? The 'serious' sense? The 'make-believe' sense? In neither of the two. Here we have again that super-sense of the concept which models itself on both the others without satisfying either. We are led to suspect that the Pickwickian fallacy is being committed—that fallacy whereby one gives a familiar word a new meaning but at the same time illicitly cashes in the old.

There is one objection to Hume's position which is so very

<sup>1</sup> Of course not! He means that all men *must* die that it is *necessary* that every man should die, that it is *inconceivable* that I will not die.  
J. A. B.

<sup>2</sup> A. J. Ayer, *Language, Truth, and Logic*, chapter 5, pp. 96-97 (*Doctr*)



obvious and so very trenchant that I am surprised it has not been seen to be decisive. In insisting that knowledge of universal synthetic connections is impossible, the sceptic is himself guilty of advancing a universal synthetic proposition. He falls prey to the familiar *tu quoque* argument. However false Aristotle's persuasion may be, that knowledge of universal synthetic *a posteriori* connections is available to us, it will not be argued that his view can be shown to be self-contradictory. Yet self-contradictory it would have to be if Hume's position, being the denial of Aristotle's, were to be accredited as analytic. In default of that, Hume's position is synthetic, and the sceptic is hoist with his own petard. For if it were in fact true that knowledge of universal synthetic connections were radically impossible, then any knowledge of that truth—itself a case of universal synthetic connection—would likewise be impossible.

The *tu quoque* rejoinder bedevils Hume's position at an even deeper level than we have noted. Let us examine his argument at close quarters. Knowledge of universal synthetic connections is held to be impossible. On what grounds? What are the premises of Hume's argument? First, that the opposite of a universal synthetic proposition can always be imagined, and second, that whatever can be imagined is possible. This last premise is our "established maxim in metaphysics . . . that nothing we imagine is absolutely impossible". In the context of common discourse the maxim has been seen to be either true and analytic or synthetic and false. Hume requires that it be both synthetic and true. Thus Hume's argument urging that there can be no knowledge of universal synthetic connections includes as one of its premises a universal synthetic proposition. This proposition, his beloved maxim, must itself be known to be true if his conclusion is to be demonstrated. But if the maxim is known to be true, then at least one universal synthetic proposition can be known to be true, and Hume's conclusion must be false. This result confirms our suspicion that Hume is guilty of the Pickwickian fallacy. He cannot allow his maxim to be synthetic. How, then, can he make it analytic? Very easily, if he assigns to 'imagine' its 'serious' sense. But there is no future for him in that option. 'Imagine' must be assigned its pictorial or make-believe sense. With that assignment, how can the maxim be construed as analytic? There is only one way. A super-sense must be assigned to the concept of possibility. For it is certainly not an analytic truth that whatever can be imagined in the pictorial or make-believe sense is possible ('possible' retaining its familiar meaning). What precisely is this super-sense

which must be assigned to 'possible' ? Can something simply be *defined*, by fiat, as possible in this super-sense, i.e. as logically possible, if it can be imagined in the pictorial or make-believe sense. What could be more jejune or futile ? It is imperative for Hume that his maxim fulfil a double function—it must have the logical status of an analytic proposition but it must also have all the push and power of a synthetic proposition. He is thus caught up in his own cross-purposes. 'Possible' must be given a new meaning, and at the same time it must retain the old meaning. A new meaning is needed if the maxim is to be analytic ; but the old meaning is required if the maxim is to be employed to prove that I am in error when I suppose that it is completely impossible for me to lift the Great Pyramid <sup>1</sup> The maxim is certainly designed to establish that the exploit is really possible—it is flung in the teeth of all our deepest convictions of common sense. In addition, then, to assuming a new meaning, 'possible' must hold fast to its old one. Hume's super-concept of possibility thus proves to be a pseudo-concept, uniting into one what cannot be united

In convicting Hume of the Pickwickian fallacy, we have failed to lay bare the underlying source of his concept-muddle. That source can never be grasped unless we discover the origin of his ruling axiom that 'nothing we imagine is absolutely impossible'. Hume brandishes that axiom like a sword, but it is a weapon not of his own making. "Established . . . in metaphysics", it comes to him as a legacy from earlier thought. What exactly is its provenance ? The maxim certainly cannot be found in Aristotle, and I can find little reason for imputing it to Plato. I venture to suggest that its origin lies in the school-metaphysics of the medievales. One has but to provide the roughest draft of scholastic ontology to see that it continues to preside over Hume's reflections. There are two types of being, necessary being and contingent being. Every item in the world and, indeed, the world itself is a contingent being. Everything perceived by the senses can be conceived to have been otherwise than what it is. Everything is possible. The only limit to the range of possibility is the law of contradiction, i.e. logical impossibility. The human mind cannot apprehend any natural or rational necessity explaining why what is, is as it is or even why it is at all. There are an infinite number of possible worlds that can be imagined. One of those possible worlds is that world which is exactly the same as our present world with but one exception—cows have the power to jump

<sup>1</sup> Cf., "As there is only a logical necessity, so there is only a logical impossibility", *Tractatus Logico-Philosophicus*, 6. 375

over the moon. This possible world differs from our actual world in no other respect whatever. Even the cows of this possible world cannot be distinguished by any physical examination from the cows in our own pastures. God could have willed, and indeed he may choose to will in the future, that this possible world should be the actual world. The future is thus entirely open. Modern ontology, following Hume, models itself on scholastic metaphysics. In the former as in the latter the distinction between necessary being and contingent being is fundamental—only now, having lost confidence in the arguments for the existence of God, the modern philosopher cannot find any application for the concept of a necessary being. This is not to say that its correlative, the concept of contingent being, is also abandoned. By no means! Everything that exists is now seen as contingent: to be is to be contingent, but contingent on nothing. Hence 'nothing we imagine is absolutely impossible'.

I am proposing that the origin of Hume's maxim and, more generally, of his scepticism (the one entails the other) is to be found in the medieval concept of contingency and, beyond that, in the scholastic concept of being. Yet even beyond Hume's concept of being ('being' understood as radically contingent) there lies still a deeper source to his scepticism—in his concept of knowledge. I have said that for Hume the proposition 'knowledge of universal synthetic propositions is impossible' is itself a synthetic proposition. I am prepared to retract that statement. A strong case can be made for the view that by assigning a super-sense to 'knowledge' Hume renders the proposition analytic. It is that Pickwickian concept of knowledge that we must now examine, for in laying it open to inspection we shall discover, finally, how he came to lose his way. Even more important, we shall find Hume himself indicating to us the trail, later to be blazed by Wittgenstein, which promises to get us out of the woods.

Like so much else in Hume, his super-concept of knowledge does not originate in his own investigations. Here again he accepts a legacy from the past, adopting the "method of expression" of "those philosophers who have . . . defined [knowledge] to be *that evidence which arises from the comparison of ideas*". We are reminded at once of Locke for whom knowledge is the perception of the agreement or disagreement of ideas and, back of Locke, Descartes with his *inspectio mentis* brought to bear on the clear and distinct ideas of the intellect and, back of Descartes, Plato and Aristotle for whom it is the faculty of *noûs* that grasps the first principles of cognition. Orienting himself by a highly rationalistic standard of knowledge, it is not surprising that Hume

is driven to scepticism: the common evidence advanced to support our convictions so seldom comes up to scratch! When Aristotle in the *Posterior Analytics* argues in behalf of the natural understanding in its persuasion that universal empirical connections are open to cognition, he disappoints us by continually enlisting examples from mathematics to illustrate the scientific enterprise. If not logical necessity, then at least geometrical necessity is proposed as the model to which natural necessity is expected to assimilate itself. Locke was the first to see that if Euclidian geometry be adopted as the model *par excellence* of cognition and, in particular, of cognition of necessary connections, then the concept of necessary connection will find little application in our empirical researches. It is vain to suppose, as Aristotle supposes, that the kind of *inspectio mentis* which obtains when we apprehend the truth of the proposition 'no two straight lines can enclose a space' is also to be found when we apprehend the truth of the proposition 'All men are mortal'. The two things are very different, and the second cannot be assimilated to the first. On the basis of the *inspectio mentis* standard of knowledge, which Hume retains, it follows virtually as an analytic truth that universal synthetic *a posteriori* propositions cannot be verified. But must we accept that standard? Is it the true standard?

There is a remarkable passage in Hume which is so pregnant with consequences, so unwittingly prophetic in its import, that one cannot but marvel as one reads it. In this passage Hume confesses that the concept of knowledge which he has relied upon to establish his sceptical conclusions is very definitely *not* the standard of knowledge which we employ in our natural inquiries: he admits that he has been using familiar words in unwonted senses, he even seems to suggest that if we "preserve the common signification of words" then we are entitled to assert that we are in possession of "proofs . . . entirely free from doubt and uncertainty" which verify universal synthetic *a posteriori* propositions. It is difficult to believe that Hume understood the passage the way we understand it today, after Wittgenstein. In any event, I have little doubt that it will come to be regarded as a *locus classicus* in modern philosophy.

Those philosophers who have divided human reason into *knowledge and probability* and have defined the first to be that *which arises from the comparison of ideas* are obliged to comprehend all our arguments from causes or effects under the general term of probability. But though everyone be free to use his terms in what sense he pleases, and accordingly in the precedent

part of his discourse I have followed this method of expression, 'tis however certain that in common discourse we readily affirm that many arguments from causation exceed probability and may be received as a superior kind of evidence. One would appear ridiculous who would say that 'tis only probable that the sun will rise tomorrow or that all men must die, though 'tis plain we have no further assurance of these facts than what experience affords us. For this reason 'twould perhaps be more convenient, in order to preserve the common signification of words, and mark the several degrees of evidence, to distinguish human reason into three kinds, viz *that from knowledge, from proofs, and from probabilities*. By knowledge, I mean the assurance arising from the comparison of ideas. By proofs, those arguments which are derived from the relation of cause and effect and which are entirely free from doubt and uncertainty. By probability, that evidence which is still attended with uncertainty<sup>1</sup>

I need only add that if we are prepared to follow Hume as far as this we shall have no hesitation in dropping his distinction between knowledge and proofs: that distinction has no warrant in "the common signification of words". We shall not rest content with saying merely that we have "proofs . . . which are entirely free from doubt and uncertainty" of the fact that 'all men must die:' we shall also say that we have knowledge of that fact as well.

#### *Retrospective Note*

Our effort in this paper to employ the Wittgensteinian elenchus to restate universal synthetic propositions and, in particular, the principle of causality will strike many readers as highly irregular and paradoxical. Is not Wittgenstein the sworn enemy of metaphysics, and have we not learned from Hume and Kant that the principle of causality is the prime example of a metaphysical proposition<sup>2</sup> The paradox is resolved once one realizes that the concept of a metaphysical proposition undergoes a profound change in Wittgenstein's thought. The tell-tale mark of a metaphysical proposition proves to be, for Wittgenstein, the presence of a Pickwickianism. Whatever else might be said against the principle of causality, of Pickwickianism it cannot be convicted!

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<sup>1</sup> *Treatise*, Bk I, Part III, section 11, *Of the Probability of Chances*, p. 124. Hume's italics

## V.—MALCOLM AND MOORE'S REBUTTALS

BY JAMES D. CARNEY

IN a recent critical notice which appeared in this journal, Mr. Norman Malcolm restates reasons for his view that Moore's defence of Common Sense "in so far as it is an interesting and tenable philosophical position" is a defence of ordinary language.<sup>1</sup> In this paper I argue that to represent the example which Malcolm cites of Moore's defence of Common Sense in this way neither makes it a tenable philosophical position nor does it represent what Moore understood himself to be doing. In the last section I state how I believe this defence is to be understood, but I conclude that though this defence is interesting, important, valuable, and, in the sense that Moore does not beg the question, tenable, it is not successful.

### I

This passage which is found at the end of the sixth of Moore's 1910-11 lectures is selected by Malcolm as showing "Moore's defence of Common Sense at work":

If Hume's principles are true, then, I have admitted, I *do not* know *now* that this pencil—the material object—exists. If, therefore, I am to prove that I *do* know that this pencil exists, I must prove, somehow, that Hume's principles one or both of them, are *not* true. In what sort of way, by what sort of argument, can I prove this?

It seems to me that, in fact, there really is no stronger and better argument than the following. I *do* know that this pencil exists; I could not know this, if Hume's principles were true: *therefore* Hume's principles, one or both of them, are false. I think this argument is as strong and good as one as any that could be used: and I think it really is conclusive. (pp. 119-120)

Having cited this passage, Malcolm then asks the question. "Now what can be the value of this apparently question-begging rebuttal of Hume?" He writes that philosophers like Hume who maintain that we do not know material object propositions to be true must be holding that the notion of knowing a material object proposition to be true is self-contradictory and "perhaps

<sup>1</sup> Since all the quotations from Malcolm's article appear on pp. 96 and 97 of *MIND*, January 1960, I will not give page references. All the quotations, thus, which are followed by page references are from *Some Main Problems of Philosophy*, Moore's 1910-11 lectures.

without fully realising it" that a sentence such as "I know that that thing sticking up in the garden is a shovel" is self-contradictory. "The important function of Moore's rebuttal" is simply to remind us that such sentences have a "correct use in ordinary discourse" or, as Malcolm writes a bit later, "can be correctly used to make a true statement". For to understand that the philosopher's claim is mistaken "... it is sufficient to realize that those sentences do have a correct use in ordinary discourse ...". Thus Moore was not begging the question since "his very point was that it is really not open to question that such sentences have a correct use." Malcolm concludes that "in so far as it is an interesting and tenable philosophical position . . . [it] is merely the assertion, in regard to various sentences, that those sentences have a correct use in ordinary language".

From what Malcolm writes one gets the impression that the rebuttal found in the quoted passage is addressed to philosophers who maintain that we do not know material object propositions to be true. But the passage contains what Moore calls his argument to prove that Hume's principles are false. Earlier in the sixth lecture Moore points out that many philosophers argue that if Hume's principles are true, then none of us ever knows of the existence of any material object, and they argue that since Hume's principles are true, none of us know that any material object exists. Moore then considers the question whether if Hume's principles are true, we could know of the existence of any material object. The first part of Hume's first principle and important part for Moore is that we can never know of the existence of anything which we have not directly apprehended. According to the philosophers who hold that Hume's principles are true, and, for that matter, according to Moore himself, the only existing things which any man does directly apprehend are his own acts of consciousness and his own sense-data. But since, writes Moore, a material object is "an object which has shape and is situated in space, but which is *not* similar, except in these respects, to any of the sense-data which we have ever directly apprehended" (p. 119), it follows that if Hume's principles are true, we can never know of the existence of any material object. But are Hume's principles true? The passage Malcolm selects is Moore's "proof" to show that Hume's principles are "false" (p. 119). Moore's rebuttal is thus in substance addressed to the view that we can know something exists only if we directly apprehend it.

Two questions come to mind (1) Is Moore's rebuttal of Hume's principles also a rebuttal of the view that none of us ever knows

of the existence of any material object? (2) Is the view that none of us ever knows of the existence of any material object the same as the view that we do not know material object propositions to be true? The latter view, as we have seen, is what is suggested by Malcolm as the object of Moore's rebuttal. If the answer to both of the questions is "yes", then it would be proper to consider Moore's rebuttal as addressed to the view that we do not know material object propositions to be true.

(1) As we have seen, Moore does not intend to prove false the claim that we do not know that material objects exist. but the reasons which are given by some philosophers for our not knowing that material objects exist. Now ordinarily to prove false the reasons which support some conclusion is not to falsify the conclusion or prove false other reasons which might be given for the conclusion. But in this context this is not the case. There are, of course, a number of different reasons which are given for our not knowing that material objects exist, for example, the dream argument or the argument that since I am sometimes deceived, I might always be deceived. But if it follows from each that we cannot know that material objects exist, then each can be refuted in the manner in which Moore refutes Hume's principles. For his argument against Hume's principles is that if they were true, then one cannot know that the pencil he holds in his hand exists, and since he knows that this pencil exists, it follows that Hume's principles are not true. As one can see, Moore's argument would equally prove false any premise from which it would follow that he cannot know his pencil exists. Similarly, the premise in Moore's argument, if true, would prove false the conclusion of the above arguments, *i.e.* we cannot know that any material object exists. Thus the way in which he would prove false Hume's principles is the way he would go about proving false the conclusion drawn from Hume's principles and the way in which he would prove false any other premises which might be offered in support of this conclusion. So if he has successfully proven false Hume's principles, he has *ipso facto* proven false any other premise or premises from which the conclusion might follow and has in addition proven false the conclusion. The answer to the first question is thus "yes".

(2) The answer to the second question is that in one sense the two statements are the same and in another they are not. Let me explain.

In the lecture from which the rebuttal which we are considering is taken Moore points out that there are 'many philosophers' who deny that we can ever know of the existence of any material



objects but who think they are not denying our knowledge of material objects (p. 113). Now this certainly sounds curious. But Moore goes on to explain that what we know when we know that a material object exists is that there exists something which must have certain characteristics. In the first lecture he says that material objects are different sorts of things from acts of consciousness, are situated in space, and exist when we are not conscious of them (p. 9). In the lecture we are considering, as I have already indicated, he says that what he means by "material object" is "an object which has shape and is situated in space, but which is *not* similar, except in these respects, to any of the sense-data which we have ever directly apprehended" (p. 119). In the next lecture he characterizes material objects in this way: they occupy space, they are neither sense-data nor composed of sense-data, and they are neither in the mind nor acts of consciousness (p. 132). Now the "many philosophers" who think that they are not denying our knowledge of material objects when they are, are those philosophers who maintain that to know of the existence of material objects is "*the same thing*" as to know of the existence of sense-data (p. 113). But since material objects are not sense-data, they do in fact deny that we can know of the existence of material objects.

What has this to do with whether the two above formulations are the same? If Moore is correct, "many philosophers" who deny the existence of material objects would not maintain that we do not know material object propositions to be true, such as, to use Moore's example, "This pencil exists". That is, they would deny that we know of the existence of things which occupy space, are not composed of sense-data, etc., but would not deny that we can know that a pencil exists, for they would maintain that to know a pencil exists is the same as to know that sense-data exist. "The pencil, so far as you mean by the pencil something which you know, . . ." consists of sense-data (p. 114). In this sense then, the two formulations are not equivalent, since one could deny that we know of the existence of things which are not composed of sense-data, etc., and not deny that propositions like "I know that this pencil exists" are true. On the other hand, if you understand words like "pencil" to refer to what Moore calls a "material object", then it makes little difference which way the philosopher's view is formulated. For the denial that we can know of the existence of material objects, *i. e.* things which are not combinations of sense-data, etc., would imply that such a proposition as "I know that this pencil exists" is false if the word "pencil" refers to something which is not a combination of

sense-data, etc. Similarly, if it were true that such propositions understood in this way are always false, it would follow that we could never know of the existence of things which Moore calls "material objects".

## II

Since, as I have argued, Moore's rebuttal serves equally as a rebuttal of the claim that we cannot know that material objects exist as well as a rebuttal for any reasons which might be given in its defence, and since one can represent the claim that no one can know that material objects exist as the claim that we do not know material object propositions to be true if one understands material object propositions to refer to what Moore calls "material objects", is it correct, as Malcolm says, that "in so far as Moore's rebuttal is an interesting and tenable philosophical position" it is to be understood as a reminder that material object propositions, such as "I know that that thing sticking up in the garden is a shovel", can be "correctly used to make a true statement"?

When Malcolm says that his representation of Moore's rebuttal is a tenable philosophical position, I take it that this involves Malcolm's believing that Moore's rebuttal, so understood, is not a case of begging the question and that it shows that the philosopher's claim is mistaken. In this section I will argue that if Moore's rebuttal is so represented, it neither shows that the philosopher's claim is mistaken nor escapes the charge of being an instance of begging the question.

Malcolm writes that all that is necessary to understand that a view such as Hume's is mistaken is to realize that such sentences as, "I know that that thing sticking up in the garden is a shovel", have a correct use in ordinary discourse. Now all philosophers who deny that we can ever know of the existence of any material object would, I believe, admit that in fact we do use such sentences. However, Malcolm suggests that if a sentence can be used correctly, it cannot be contradictory. And a few sentences later, Malcolm adds that Moore was reminding us that certain sentences "can be correctly used to make a true statement". But even here, according to Moore, "many philosophers" who deny that we can know that material objects exist would insist that such sentences are not contradictory and can be correctly used to make true statements since to say that we know that the thing sticking up in the garden is a shovel is to say nothing more than that we know something about sense-data. Thus if Malcolm is correct and Moore's rebuttal is to be understood merely as a reminder that

such sentences are not contradictory and can be correctly used to make true statements, then his rebuttal would not meet the claim of "many philosophers".

However, reminding the philosopher that such sentences are not contradictory and can be correctly used to make true statements would constitute a denial of the philosopher's claim if it were true that from the fact that such sentences can be correctly used to make true statements in ordinary discourse *it follows* that we know that there exists things not composed of sense-data, things which exist unperceived, etc. But is this true? Now I believe that this is not true, and I believe that Malcolm also holds that this is not true. For if it were true, then from the fact that certain sentences are not contradictory and can be correctly used to make true statements it would follow, depending on the sentence, that either we know that things exist which are not combinations of sense-data, exist unperceived, etc., or that such things do exist. Now if it were true that such things exist and we know that such things exist, then various forms of Idealism and some forms of Realism would be false. Thus from the fact that certain sentences can be correctly used to make true statements in ordinary language, it would follow that several philosophical views would be false. I do not believe that Malcolm holds that this is possible even though some critics attribute this to him. In addition I believe that Malcolm would agree that pencils can neither be nor not be composed of sense-data because it makes no sense to speak of pencils as so composed. If this is so, then the word "pencil" cannot refer to what Moore calls "material objects", for he says that material objects are things not composed of sense-data. Now to say that pencils are not composed of sense-data could mean that it makes no sense to say that they are. But this is not how Moore's saying that pencils are not composed of sense-data is to be treated in this context, for his claim is the denial of the view of philosophers, and they do not maintain that it makes sense to say that pencils are composed of sense-data but that they are so composed. Thus if to say pencils are composed of sense-data is nonsense, then when Moore says that they are not, this is also to utter nonsense. Consequently, it cannot be true that the word "pencil" refers to something which is not composed of sense-data. I believe a similar analysis holds for the phrases "occupies space", "has shape", and "exist unperceived" as they appear in this context.

Thus if Moore's rebuttal is understood merely to be a reminder that certain sentences are non-contradictory and can be correctly used to make true statements, then not only does his rebuttal not

show that the claim of "many philosophers" is mistaken, but it is a reminder to them of what they agree is true. On the other hand, if Moore's rebuttal is to deny the claim of these philosophers, then it would no longer be a tenable philosophical position since a necessary condition to show that the philosopher's position is mistaken is for words like "pencil" to refer to what Moore calls "material objects".

However, there is another difficulty with Malcolm's interpretation. If his interpretation is correct, Moore would beg the question.

If to remind the philosopher that certain sentences can be used correctly is sufficient to show that the philosopher's claim is mistaken, then, Malcolm argues, Moore would not be begging the question, since "it is not really open to question that such sentences have a correct use". I am not sure if Malcolm wishes the phrase "to make true statements" to be added to this last quoted sentence. If so, then if words like "pencil" are understood to refer to what Moore calls "material objects", it would be open to question, for this is what the philosopher denies. But let us at this time interpret Malcolm as maintaining that just the fact that certain sentences have a correct use is sufficient to refute the philosopher and that since this is not open to question, Moore would not be begging the question.

It is true that it is not open to question that such sentences have a correct use, but if this is to constitute sufficient grounds for the falsity of the claim of those philosophers whom Moore wishes to rebuke, then two other things must be true. (i) Words like "pencil" and "shovel" must be understood to refer to what Moore calls "material objects". And (ii) it must be true that if a sentence has a correct use, then it cannot be contradictory. If (i) were not true, then, as I have already argued, the "many philosophers" who deny that we can know that material objects exist could agree not only that such sentences can be used correctly but that they can be used to make true statements without this admission being incompatible with their denial. If (ii) were not true, then from the fact that certain sentences have a correct use, assuming (i) to be the case, it would not follow that the philosopher's claim is false. But for the philosopher we are considering, if (i) is assumed to be true, it is open to question whether (ii) is true. In fact the very claim of the philosopher is that one cannot know that a material object exists, for, as we have already seen, if Hume's principles are true, one can only know what can be directly perceived. And if the word "pencil" refers to what Moore calls a "material object", since a material

object is something one cannot directly perceive, it follows that to say "I know the pencil exists" is to say "I directly perceive something which I cannot directly perceive"—a contradiction. Thus in order for the reminder that certain sentences can be used correctly to imply that the philosopher's claim is mistaken, it must be true that sentences like "I know the pencil exists" (again assuming that the word "pencil" refers to what Moore calls a "material object") are not contradictory. But this is what the philosopher denies. Consequently, in order for the reminder to meet the philosopher's claim, it must involve asserting what is at issue.

### III

Is there a way to represent Moore's proof which would remove the charge of his begging the question? In this section I will suggest such a way. And I will show how his rebuttal should be understood if it is not a case of question begging.

The passage quoted from Moore's lecture contains his proof that Hume's principles are not true. The argument, as we can see, is that if Hume's principles were true, then Moore could not know that the pencil which he holds in his hand exists. But since he knows that this pencil exists, it follows that Hume's principles, one or both of them, are false. Now Moore speaks of this argument as a *reductio ad absurdum* argument. But this is strange, since in such an argument one expects to be presented with consequences which are not readily seen and which one is inclined to think false. But as Moore points out his argument is not convincing to those who believe that Hume's principles are true and believe, as a consequence, that Moore does not know that this pencil exists. For them it is merely a statement of their argument, and thus a strange sort of *reductio ad absurdum*. However, Moore is aware of the strangeness for he goes on to say that to those who hold that Hume's principles are true, his argument "seems like begging the question". Now Moore says "seems like" because he, I take it, does not believe that he is begging the question. And, in this lecture, he proceeds to show that he is not begging the question because his argument "really is a good and conclusive argument" (p. 120). Moore's argument is I do know that this pencil exists; therefore, Hume's principles are false. The argument is a "good and conclusive argument" because, first of all, the conclusion does follow from the premise. If Moore knows that a pencil exists, and a pencil is a material object, then it is false that we can only know that which we directly

perceive (Hume's first principle), for we can only directly apprehend acts of consciousness and sense-data. Secondly, his argument is "good and conclusive" in that, according to Moore, he knows the premise to be true.

Those who are inclined to make the charge that Moore was begging the question would, of course, admit that if his argument is good and conclusive, then he was not begging the question. And they would admit that his argument is good in the sense that the conclusion follows from the premise. But they would object to Moore's saying he knows the premise to be true. The premise, it will be recalled, is "I do know that this pencil exists". But this is affirming what is at issue. For what is at issue is Hume's principles, and the first principle states that we can know only those things which we can directly apprehend. To assert that we can know that this pencil exists is to assert that we can know things which we cannot directly apprehend.

There are several things in Moore's rebuttal which make it similar to begging the question; but, if Moore is correct, there is an important difference which would raise doubt that he is begging the question. Moore's argument bears a resemblance to begging the question in that his premise is a restatement of what is at issue and his argument is not convincing to those who adhere to that which is supposedly being rebuked. But Moore's argument differs in this important respect. Near the end of the lecture Moore writes:

But whether the exact proposition which formed my premiss, namely I do know that this pencil exists; or only the proposition This pencil exists, or only the proposition The sense-data which I directly apprehend are a sign that it exists, is known by me immediately, one or other of them, I think, certainly is so. And all three of them are much more certain than any premiss which could be used to prove that they are false, and also much more certain than any other premiss which could be used to prove that they are true (p. 125).

I would like to call attention to the last phrase "much more certain than any other premiss which could be used to prove that they are true". In order to beg the question one either affirms or denies the question *without giving reasons*. But it must be appropriate to give reasons. If it is not appropriate, then one cannot very well be censured for affirming or denying something without giving reasons. And if Moore is correct, it would be rather silly to give reasons, for any "reason" which might be given would not be as certain as that for which the "reason" would be offered. Thus though Moore's argument is not

convincing and though its premise is an affirmation of that which is at issue, if Moore is correct, it differs from an instance of begging the question enough, so that we do not know what to call it

I have suggested that there are strong reasons for Moore's rebuttal not being an instance of begging the question. But in order for this to be true, Moore's premise must be one for which it is silly both to ask for reasons which could be used to prove that it is true and to entertain the possibility that there could be reasons which could be used to prove that it is false. Moore thought that holding up his wooden pencil provided the circumstances for uttering just such a proposition, "I know that this pencil exists". In such circumstances with the pencil before him, what could possibly lead him to say that he does not know that this pencil exists or what could lend support to his knowing that this pencil exists? Whether or not Moore is correct is not what I want to stress but rather that Moore needed to do this in order to make a claim which would be, as he thought, immune to support or rejection. Thus rather than Moore's rebuttal being a reminder that certain sentences can be used correctly to make true statements, it seems that his rebuttal consists in producing something in such circumstances that it would be out of order to give reasons either for why he knows it exists or why he knows it does not exist.

At the end of the next lecture Moore points out that Hume's principles are either self-evident truths or empirical inductions. Since, as Moore argues, they are not self-evident truths, for a self-evident truth is one which you can see must be true by merely considering the terms, such as "things which are equal to the same things are equal to one another", they must be empirical inductions. "But, this being so, it follows that no such general principle can have greater certainty than the particular instances upon the observation of which it is based" (p. 143). The fact, thus, that Moore knows that this pencil exists is sufficient to falsify the generalization. If Moore is correct, then the way in which he goes about falsifying Hume's principles, providing a counter-example, is the *only way* in which one could falsify them, since they are empirical generalizations. And if this is so, then it seems odd to say that this is begging the question. Furthermore, we can see the importance of producing an example.

In summary Moore, I believe, makes a number of mistakes in his rebuttal. Hume's principles are not empirical inductions. More importantly, Hume's principles cannot be shown to be false, though, I believe, they can be met in other ways. If I am correct in this, then one cannot represent Moore's rebuttal in such a way

that it will show that the philosopher's view is false. Consequently, Moore's rebuttal, though it is an interesting, important, and valuable philosophical position, cannot be a successful position. But though he might with justification be censured for these things, it does not seem that he is begging the question.

Furthermore, I have argued that Moore's rebuttal should not be represented as a defence of ordinary language. This representation does not make it a tenable philosophical position. Also it does not represent what Moore understands himself to be doing. For in order for his rebuttal, so understood, to be even a denial of the view he intends to refute, words like "pencil" must refer to what he calls "material objects." Now for Moore this is the case, so from the fact that certain sentences can be correctly used to make true statements it follows that the view that we cannot know that material objects exist is false. However, as I have argued, if this is what Moore was doing, it would be an instance of begging the question. But as Moore looked at his rebuttal he was not begging the question but providing a "good and conclusive argument." And this, as I have argued, necessarily involves producing an example of something in such circumstances that there can be no reason which would make it more certain that one knows it exists or that one cannot know it exists.

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## VI.—DISCUSSIONS

### ILLUSIONS AND DREAMS

#### I

It is sometimes said that one can never know whether or not one is awake. Malcolm's way of dealing with this problem is to say that the sentence "I am not awake" is a senseless sentence and does not express a possibility that can really be thought. There are not two things for me to decide between, one that I am awake and the other that I am not awake. "There is nothing to decide, no choice to make, nothing to find out"<sup>1</sup>

It seems to me that the words "I don't know whether or not I am awake", cannot express a doubt at all. It is perfectly true that another person can know of me that I am awake, but this does not imply that it makes sense to say that this is something that I can be said to know about myself. I think that the strongest temptation to believe that the words "I know that I am not asleep" make sense comes from the observation that the words "I know that he is not asleep" have a perfectly good use. We find it hard to believe that just changing a pronoun in an assertion can transform that assertion from a meaningful one into nonsense.

But this lack of symmetry between the first person uses of sentences and their third person uses should by now be familiar to us and no longer be a source of trouble. What is surprising is the frequency with which philosophers, noticing the lack of symmetry here, manage to give a completely inverted account of its character when the sentences involved are psychological sentences or concern the mental. That account would make it appear that there is a problem about knowing what is going on in the minds of others but no problem about what is going on in our own mind. That, for example, the problem is how one can ever know that another man has had such and such a thought. That I know that I have had such a thought or am having such a thought is taken for granted. So the lack of symmetry is noted all right, but this account is completely reversed. "I know I have a headache" is an assertion without a clear sense, whereas "I know that he has a headache" is a perfectly normal and frequently used assertion. It is a bit surprising that only recently have philosophers turned their attention to this problem of our knowledge of *our own* minds.

Let us note first of all that in the case of "I know that he is awake", it makes perfectly clear sense to ask "How do you know that he is awake?" But if you were to ask me "How do you know that *you* are awake?", what could I possibly say by way of an answer? Suppose I were to say, "Well, I have a headache, and I never have a headache except when I am awake". Would this get

<sup>1</sup> *Dreaming* by Norman Malcolm, Routledge and Kegan Paul, London, p. 118.

us anywhere? A man who can ask "How do you know that you are awake?" can also ask me how I know that I have a headache. How do I know? Is there anything to say here? I can only say that we do not use the expression "I know I have a headache", so no one can sensibly ask us *how* we know. Neither is there a use for the expression, "I know that I am awake".

This comparison between "I am awake" and "I have a headache" is worth pursuing a little further. A man can suppose that I am lying when I say "I have a headache", but he cannot suppose that I am lying when I say "I am awake". Why not? What is it to find out that a man was lying when he said that he had a headache? Is it not just to learn that he subsequently admits that he was not telling the truth in the first place? If he insists that he had the headache and betrays no sign of attempting to deceive us we cannot logically insist that what he says is not the case. I am supposing that we are dealing with a man who had a mild headache, one which would not involve his contorting his body or crying in agony. In such a case the man's saying that he had a headache would be the criterion of his having a headache, and his subsequent declaration that he was lying would be the criterion that he had lied. But now we see why it is senseless to suppose that a man was lying when he said "I am awake". If it were like saying "I have a headache" in this respect, then we might suppose that we might find out that he was lying if he were subsequently to admit that what he first said was not true. That is, we suppose that he says "You know, when I told you I was awake, I really wasn't". What sense would this make? How could a man claim to have told us that he was awake, at a time when he was not awake? He might have said these words in his sleep, but that could hardly constitute a lie.

Now what conclusion can we draw from this observation that a man cannot be said to have lied about whether or not he was awake? One conclusion is that a man cannot be said to have been truthful about the matter either. How odd it would be were any one to say "X tells us that he is awake and he is being truthful about the matter". Where you cannot lie you cannot be truthful either. Where the distinction between lying and being truthful does not exist neither does the distinction between giving a right and giving a wrong answer to a question. "Are you awake?" does not have a right answer, any answer is as good as any other. But if any answer is as good (or right) as any other, surely there is no question, "How do you know that you are awake?", for the question "How do you know?" *can* only sensibly be asked where one can suppose that the answer which has been given to a question *might* have been wrong. When, that is to say, one wishes to know how it happens that one is able to give the right answer. But if one cannot give a wrong answer to a question what sense does it make to ask "How do you know (or come to know) the right answer?" It makes no sense to ask, "How do you know that you are awake?" But if it

makes no sense to ask "How do you know?" then it also makes no sense, in this case, to say, "I know". It makes no sense to say "I know that I am awake".

Scepticism in this area draws a good deal of its force from the very natural idea which we have of being awake as an inner state of which we can take note by means of a faculty of inner vision, introspection. What I am trying to do is to show that this picture of the inner state will not do, i.e. that the analogies upon which this picture rests break down at the crucial points. This supposed inner state cannot be like a sensation or feeling such as toothache because I can be about that, I can attempt to deceive you about my sensations and feelings, again, if one is following the model of sensations and feelings one cannot maintain that it makes sense to ask "How do I know that I am awake?" as one cannot ask "How do I know that I have a toothache?"

Now, of course, one can sometimes ask oneself whether one is really sure about his "inner states". One sometimes can be in doubt as to whether one is not deceiving oneself about one's motives, emotions, attitudes or even feelings (in another sense of that word, e.g. "feelings of regret"). Was I feeling regret, for example, or was it really self pity? Did I act out of patriotism, as I have been telling the story, or was it more just cowardice that moved me to do what I did? But if one can wonder about one's motives or feelings in any particular case, one can also suppose, in each such case, that another person might know the truth about oneself even if one does not himself know. So another person might have known that I was not really angry, but jealous, while I have been telling myself (and others) all along that my feeling was one of anger. I have deceived myself but perhaps I have not deceived anybody else. It is curious that in philosophical discussions of these problems it is often overlooked that the possibility of knowledge and doubt concerning our own "inner states" is tied logically to the possibility of knowledge and doubt on the part of others. It is the converse which is not true. The theory of "privileged access" is just the reverse of these truths.

But here again the analogy between "Am I angry?" and "Am I awake?" breaks down. Could we suppose that someone else might be in a position to correct my own "view" on the matter? Could someone else reveal to me that I am awake as he can show me that I am jealous. Could I be mistaken in thinking that I am awake? Perhaps I could ask someone, "Do you think that I am awake?" Of course not! Being awake is not like having an emotion or a motive or a feeling in this way.

The view that being awake is some "inner state" the presence of which I can take note of must end up by giving away all claims to the support of the analogies which have been discussed above. It cannot be like toothache because I cannot wonder whether I have *that*. It cannot be like patriotism because, though I can wonder

whether I really am patriotic, it also makes sense to suppose that another may know better than I do. But I cannot suppose that another may know better than I do whether or not I am awake. So one must end up by thinking of being awake as unique in all of these ways if one is to go on holding that one can significantly wonder whether or not one is awake.

But perhaps the strongest source of the Cartesian kind of scepticism in connection with dreaming comes from the undeniable fact that we do sometimes ask ourselves "Am I awake?" and "Am I dreaming?" Let us look more closely at these situations in order to see just what support they actually do lend. Sometimes as I awake from sleep and am still not completely awake I do ask myself the question "Am I awake?" This is especially likely to happen if I awaken in rather unfamiliar surroundings. But notice the peculiarities of this question as it is now asked. In the first place, it would be absurd to ask this question of another, "Am I awake?" Where one cannot sensibly ask a question of another, one cannot sensibly claim to know the answer to that question. Further, one may ask oneself "Am I awake?", but *one cannot keep on asking it*. We would not know what to make of a man who after waking in unfamiliar circumstances asked himself the question "Am I awake?", then went about getting dressed, eating his breakfast, etc. and all of the time kept on asking himself "Am I awake?" Perhaps such a man is mad, or he is philosophizing, but the question as asked in the later hours of the morning is *no longer the same question* which was asked on arising.

Why cannot one just keep on asking oneself "Am I awake?" Is it because it is such an easy question to answer? It is rather that one does not find out or discover an answer at all! One just becomes fully awake and stops asking the question. One does not make an experiment or test which *could* give the wrong answer. Rather one becomes fully awake and cannot give any further sense to the question. One cannot think of an experiment or test here because the outcome of such a test or experiment is always the same. Suppose one pinches oneself. Are there two possible results here? One favouring the hypothesis of sleep and the other of dreaming? Of course not! Which would go with which?

## II

In a recent study of Malcolm's work Professor A. J. Ayer concludes that Malcolm's efforts are a total failure. This failure he says leaves us with "no alternative to the classical theory"<sup>1</sup>. He summarizes his own view as follows: "Dreams are experiences. They are mostly illusions and are found to be so by the same criteria as apply to illusions in general. Their peculiarity, by definition, is

<sup>1</sup> "Professor Malcolm on Dreams" by A. J. Ayer, *The Journal of Philosophy*, Vol. LVII, No. 16, 4 August, 1960, p. 535

that they occur to us only when we satisfy the physical conditions of being asleep. But with respect to their status as illusions this is logically irrelevant" (p 535)

The observation which particularly interests me here is Ayer's contention that these "illusions" can be found to be so by the same criteria which apply to illusions in general. We have all had the experience of having an illusion at one time or another and then discovering that we were having an illusion. Ayer seems to think that all illusions are such according to the "same" criteria. What are these criteria? "The test which a perception has to pass in order to qualify as veridical is that the information which it seems to yield shall fit in with that which is obtained from the vast majority of our other perceptions" (p 533). This is what Malcolm has called "the principle of coherence". Malcolm gives as one formulation of this principle the formulation of Descartes at the end of the *Meditations*: "I ought to set aside all the doubts of these past days as hyperbolical and ridiculous, particularly that very common uncertainty respecting sleep, which I could not distinguish from the waking state, for at present I find a very notable difference between the two, in as much as our memory can never connect our dreams one with the other, or with the whole course of our lives, as it unites events which happen to us while we are awake. And, as a matter of fact, if someone, while I was awake, quite suddenly appeared to me and disappeared as fast as do the images which I see in sleep, so that I could not know from whence the form came nor whither it went, it would not be without reason that I should deem it a spectre or a phantom formed by my brain (and similar to those which I form in sleep), rather than a real man."

Now Malcolm maintains that the coherence principle is open to a fatal objection. "The objection that should occur to anyone is that it is possible a person should *dream* that the right connections hold, *dream* that he connects his present perceptions with 'the whole course of his life'. The coherence principle tells us that we are awake if we can make these connections and asleep in a dream if we cannot. But how does the principle tell us whether we are not dreaming and making connections, or dreaming that we are? It seems to me that obviously it cannot and therefore the principle is worthless." (p 108)

Ayer admits that one might merely dream that one had applied the coherence principle successfully and therefore wrongly conclude that one was not dreaming. He does not, however, believe that this makes the "test" worthless. It only shows that it is not conclusive. He is quite convinced, he tells us, that he is awake and writing a paper. But he is quite willing to admit that he may subsequently have the experience of waking up to discover that he was only dreaming. Still, so long as this does not happen and all of his experiences are "consonant" with his now writing the paper he feels that they can be taken as evidence that he is writing the

paper. But, of course, even if one does have the experience of waking up, this experience itself may not be veridical. It may be part of one's dream. So my subsequently finding the paper unwritten might itself be part of my anxiety dream! This is Ayer's argument. In claiming that one is not dreaming one is not claiming to know this for certain but only claiming to have evidence which supports this conclusion (p. 534).

So here we have Ayer writing a paper and at the same time looking for evidence that he is writing a paper! Ayer treats the words "I am awake" as though they were a hypothesis for which we can accumulate evidence. Evidence may subsequently turn up that I am not now writing this paper. What evidence? I may wake up! But having wakened up I may wake up again and then it turns out that I only dreamt that I woke up in the first place. Let us examine this idea of "evidence" that I am now awake.

If there can be evidence for a hypothesis there can also be evidence against it. So presumably there is also sometimes evidence that I am not awake. But the whole concept of "evidence" in this context is queer. For one thing, all of the evidence is equally compatible with either "hypothesis". For Ayer claims that dreams "are not intrinsically distinguishable from the veridical experiences which we have when we are awake" (p. 532). So any item of "evidence" that we are awake can equally be taken as an item of evidence that we are dreaming. What sense does the word "evidence" have here? What sense does the word "evidence" have when a man sitting at his table and writing a paper asks for evidence that he is writing a paper? Ayer says that as long as he does not have the experience of waking up, he has the right to assume that he is awake. But waking up does not settle anything either. One could just dream that one woke up. Well, what *would* settle the matter? Ayer admits that *nothing* would settle the matter. We must just be content with gathering more and more evidence for a hypothesis which no amount of evidence could as a matter of logic, establish. In my view, this renders absurd the view that "I am dreaming" or "I am awake" expresses any hypothesis at all.

It is quite likely that Ayer is misled in his thinking here by some such reflections as the following. It often happens that we believe something reasonably enough even when it has not been conclusively established that what we believe is the case. It is reasonable to believe these things if we have some positive evidence in favour of them. And it is more reasonable the more evidence we have. But, of course, it is one thing to say that we have not yet established or confirmed the truth of an assertion and quite another thing to say that *nothing* as a matter of logic *could* be said to establish it as true or completely confirm it, or to say that *no matter what occurred the question would still be left open*. But this is just the kind of characterization which Ayer gives of his "claim" to be awake. He says that he has evidence for the claim but admits that nothing could

settle the reasonable doubt which one can always entertain concerning that claim. So the analogy with genuine empirical hypotheses and genuine empirical claims generally is no good at all here. The idea of an empirical claim such that no evidence *could* settle the matter of its truth or falsity is a contradiction in terms. Ayer pictures himself as gathering evidence, as moving towards a goal. But he gives us no idea of what it is like to arrive at this goal. We are on our way, in fact, we are nearly there, but we will not know it when we arrive and there is no way of telling that we are where we were going.

Another source of the kind of view which Ayer presents is this. There is such a thing as finding out (getting evidence) that one *dreamt* such and such, or that one *was* awake. This might lead one to think that there must also be some way of gathering evidence that one is awake or that one is dreaming. Ayer tells us that getting evidence that one is dreaming is getting evidence of the same kind that one gets that one is having any other sort of illusion or hallucination. Of course one can have reason to believe that one is having a hallucination. One can know that one is having a hallucination. Ayer says that this same kind of evidence can lead us to believe that we are awake or dreaming. But he does not tell us how the evidence for a dream would differ from the evidence for a hallucination.

Suppose I "see" pink rats eating a purple elephant. Ayer's view would be that this is determined to be a hallucination because these "experiences" fail to cohere with the rest of my experiences. But what distinguishes the evidence for "I am having a hallucination", from the evidence for "I am dreaming" according to Ayer? He says that coherence is the test in both cases. But there is a difference between dreaming that one sees pink rats and having the hallucination of seeing pink rats. If one can sensibly ask for evidence that one is dreaming what is this evidence supposed to be? The failure of my current experience to cohere with the rest of my experience will not distinguish between dreaming and having a hallucination. My waking up could at most convince me that I had dreamt and not that I am dreaming. If there is to be a difference in meaning between "I am dreaming" and "I am having a hallucination" there must be something which counts for the one and not for the other. But I cannot see what this would be. Ayer only supplies us with a criterion of coherence which tells us that the evidence for dreaming is of the same "kind" as the evidence for "other" illusions, without telling us also how it differs.

Now, of course, what distinguishes the man who is dreaming from the man who is having a hallucination or illusion is that the first man is asleep and the second man is awake. So if I can get evidence that I am sleeping I will know that I am dreaming and not having a hallucination. But what could such evidence be? That I am dreaming? That would be arguing in a circle! That my body is

disposed in a certain manner? The supposition that I know *that* is incompatible with the supposition that I am sleeping. No the idea of evidence that I am sleeping, if this is to be gathered by me rather than by a third person, is absolutely devoid of content. But surely all of this should be evident without the need for argument. No matter how odd or devious my experiences may be they never justify the conclusion "I am dreaming." A man may complain to his friend of having a hallucination but not of having a dream. "I am dreaming" has no sense as a report of an experience.

Finally, this difficulty arises for Ayer's view. It is surely possible that I might dream that I was having a hallucination. And this is surely different from having a hallucination. Let Ayer tell me how to use the criteria of coherence to find out which of the two is *now* happening to me. Waking up will not do. That will only establish that I did dream that I had a hallucination. On my view "I am dreaming that I am having a hallucination" makes no sense, where "I am having a hallucination" does make sense.

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## GASKING ON ARITHMETICAL INCORRIGIBILITY

IN his article "Mathematics and the World" Douglas Gasking maintains that "we could use *any* mathematical rules we liked, and still get on perfectly well in the business of life".<sup>1</sup> The point of this claim is to help us understand better how arithmetical propositions are incorrigible. It is argued that if the use of a particular set of arithmetical rules ever were to lead consistently to results which were incompatible with our measurements of physical objects, we nonetheless could preserve the arithmetic by altering the way we think about the physical world. Thus any set of arithmetical rules could be employed without adverse practical results if only we are prepared to make suitable adjustments in our conception of physical objects. A claimed consequence of this liberality is that any self-consistent arithmetic, including our own, is compatible with any physical state of affairs, and hence is incorrigible. "An incorrigible proposition is one which you would never admit to be false *whatever* happens: it therefore does not tell you *what* happens" (208, author's italics).

Gasking's discussion does not bring out adequately the relation between the assertions (1) that we can use any arithmetical *rules* we like without compromising our practical interests, and (2) that arithmetical *propositions* are incorrigible. Assertion (1), even if correct, does not justify assertion (2). Moreover, the reasons given for maintaining (1), if cogent, furnish grounds for rejecting (2). I wish to show why this is so.

The mistaken attempt to establish (2) by arguing (1) involves an unclear distinction between arithmetical rules and propositions. An arithmetical proposition, in Gasking's use (207), is a formula like ' $7 \div 5 = 12$ ' or ' $3 \times 4 = 12$ '. His claim then is that formulae like these are incorrigible. But such formulae have a variety of uses, and may be incorrigible in some but not in others. They (1) can represent interpretations of equations derivable within an initially uninterpreted formal system, as in *Principia Mathematica*. In this use, ' $3 \times 4 = 12$ ' usually would be considered incorrigible. They (2) can be used in classroom drills, as when a child is instructed to write ' $3 \times 4 = 12$ ' several times as an aid to memory. In such use the expression is part of an exercise and is neither true nor false; hence it is neither corrigible nor incorrigible. It is clear from the text (217, 218), however, that Gasking uses expressions like

<sup>1</sup> "Mathematics and the World". in *Logic and Language*, 2nd ser., ed. Antony Flew, Oxford, Basil Blackwell, 1953, p. 213 (author's italics; all references are in this pagination). The essay appeared originally in the *Australasian Journal of Psychology and Philosophy*, 1940, and has been reprinted also in *The World of Mathematics*, vol 3, ed. James Newman, New York, Simon and Schuster, 1956. In accordance with the extent of Gasking's argument, I henceforth write 'arithmetical' in place of 'mathematical'.

' $3 \times 4 = 12$ ' in yet another way, 3) to *express* arithmetical rules. Thus, when he refers to the rule that twelve (instead of thirteen, *eg*) is to be taken as the product of three and four, he uses either ' $3 \times 4 = 12$ ' or 'three times four equals twelve'.<sup>1</sup>

What is meant by attributing truth (or falsehood) to a proposition expressing a rule? Consider the rule expressed by (R) 'Ladies are not permitted in the choir stalls'. R might *announce* the rule (by being posted near the choir), or it might *record* the rule (by being inscribed in a book of regulations). In either case we would say R *expresses* the rule. But it would be incorrect to say R *is* the rule. The rule might be in effect without being announced by R, or even without being recorded, on the other hand R might be posted near the choir (by pranksters) even though there is no such rule. Further, expressions of rules are evaluated differently from the rules themselves. Although we might evaluate a rule as being just or unjust, effectual or ineffectual, we would not evaluate it as being either true or false. A proposition which purports to express a rule, however, is either true or false. It is true if what it expresses *is* a rule (is in effect), otherwise it is false. Thus R, in its context, is true if in fact ladies are not permitted in the choir stalls, and false if there is no rule to that effect.

Analogously, the proposition ' $3 \times 4 = 12$ ' is true if what it expresses has the force of a rule in arithmetic. If no conceivable state of affairs would lead us to consider that ' $3 \times 4 = 12$ ' had been falsified, then by Gasking's account it is incorrigible. He maintains (assertion 2) that such propositions *are* incorrigible on the basis (assertion 1) that we can use any set of arithmetical rules we choose for everyday practical affairs.

No attempt is made to *prove* that we could use any set of rules we choose for the world's practical business. Gasking rather attempts to show how we could go about preserving an arithmetical calculation which runs foul of common observation. He does this by considering how we would handle arithmetical rules which produce results different from those to which we are accustomed, given the physical world as we conceive it.

Imagine a society in which an arithmetic involving the formula ' $3 \times 4 = 24$ ' is taught to school children and used by all men of affairs. This formula is not merely a different way of expressing what we express by ' $3 \times 4 = 12$ ', the numbers three, four, and twenty-four are used in counting and measuring just as we use them. The difference is that members of this society hold that three fours are twenty-four, and write ' $3 \times 4 = 24$ ' as a correctly formed expression of the rule to that effect.

<sup>1</sup> Gasking does not formulate explicitly his conception of a mathematical rule. He attempts rather to clarify this conception by discussing the use of mathematical rules, this is the purpose of his article. The point I wish to make does not require a more explicit formulation of this conception.

Thus society inhabits our world. Its people, however, conceive physical objects to behave in a way which to us would seem queer. Floor tiles, for example, are conceived to double in area when installed. This expansion is not measurable, for measuring sticks are thought to expand just the right amount under just the right conditions to render such changes undetectable. Members of this society by hypothesis cannot observe the expansion, and have been taught not to expect to be able to do so.

Consider a builder in this society who sets about to pave a floor measuring three by four yards with flat tiles measuring one yard on each side. He first measures the floor as we would, obtaining the dimensions three by four yards. Then he multiplies these numbers by the only arithmetic he knows to obtain the product 24. This represents the area of the floor. Instead of ordering 24 tiles, however, he orders only 12, because he has learned that materials like tile double in area when used. When the 12 tiles arrive, the builder lays them down with neither tile nor floor space left over.

This queer arithmetic is not idle. Its purpose is to show the way in which arithmetical propositions more familiar to us are related to the physical world. The way our expression ' $3 \times 4 = 12$ ' is related to the world as we conceive it is said to be like the way ' $3 \times 4 = 24$ ' is related to the world as conceived in the imaginary society. There, ' $3 \times 4 = 24$ ' is held in a way which makes it unfalsifiable by any conceivable result of counting and measuring. When floor spaces and the like are found regularly to accommodate tiles totalling only one-half their calculated areas, the proposition is maintained that tiles double their areas in a way beyond detection. Similarly, as we do things, if spaces were found regularly to accommodate more (or less) tiles than would be expected on the basis of our calculations, presumably we would not relinquish ' $3 \times 4 = 12$ '. Instead we would maintain that tiles sometimes contract (expand), even though we could not detect this change by measurement.

Perhaps this reasoning can be understood better with the aid of a few symbols. Let us write

- 'p' for ' $3 \times 4 = 12$ '
- 'q' for 'tiles do not change area when used'
- 'r' for 'less than 12 tiles  $1 \times 1$  just cover an area measured  $3 \times 4$  units'

If r is true, either p or q (or some expression like p, such as ' $1 \times 1 = 1$ ') must be rejected. Illustrations like that of the builder are to prepare us to accept the following general claim: whenever a statement about the results of counting and measuring turns out to be incompatible with the conjoined assertion of a set of arithmetical rules and a statement of how we conceive objects to behave, we can always reject either the arithmetical rules or our conception about objects. In this fashion, Gasking argues that *our* arithmetic is related to the world in the way in which *any* arithmetic is related to

the world, even one in which  $3 \times 4 = 24$ . It is related in such a way that it stands to be contradicted by the results of counting and measuring only in conjunction with a specific set of conceptions about the behaviour of objects, which if we choose could be rejected instead of the arithmetic. The propositions of our arithmetic thus are claimed to be incorrigible, because they are compatible with any and every conceivable state of affairs.

But even if we are content with the drift of the argument thus far, we are not constrained to agree that ' $3 \times 4 = 12$ ' is incorrigible. If  $r$  entailed the negation of  $p$ , of course,  $p$  would not be incorrigible, it would be incompatible with the conceivable state of affairs expressed by  $r$ . Gasking argues that  $p$  is incorrigible because a statement like  $r$  could entail only the negation of either  $p$  or  $q$ , and  $q$  can always be rejected instead of  $p$ . But from the admission that either  $p$  or  $q$  could be rejected as a consequent of  $r$ , it does not follow that  $p$  is compatible with any conceivable state of affairs. If  $r$  entails the negation of either  $p$  or  $q$ , then  $p$  entails the negation of the conjunction  $q$  and  $r$ . But if  $p$  entails the negation of the conjunction  $q$  and  $r$ , then  $p$  is incompatible with a conceivable state of affairs—that described by the conjunction  $q$  and  $r$ . The state of affairs described by the conjunction  $q$  and  $r$  is a state of affairs in which the rule that twelve shall be taken as the product of three and four does not govern arithmetical calculations.

It might be objected that while  $q$  and  $r$  taken separately describe conceivable states of affairs, taken in conjunction they do not. The only basis for this objection that I can think of, however, is an appeal to the inconceivability of not- $p$ . This Gasking cannot admit, since his essay is taken up largely with considering how people who reject  $p$  might successfully calculate in their daily affairs. Another possible objection is suggested in the following passage, where he anticipates a point similar to mine.

It might have been said " $3 \times 4 = 12$ " does describe and depend on the nature of reality, because it entails a certain purely empirical proposition about what does and does not happen, namely the complex proposition "It is not the case *both* that tiles do not expand *and* that we need less than 12 tiles to pave a floor measuring 3 by 4". But I should maintain that this complex proposition is not empirical, that it does not describe anything that happens in the world because it is incorrigible (219, 220, author's italics).

My point is less sweeping. I do not claim that  $p$ , or any proposition it entails, describes something which happens in the world. As Gasking uses ' $3 \times 4 = 12$ ' and similar expressions, they do not describe anything. They *express* rules of calculation. And rules of calculation, like other rules, are not among things of which we would say that they happen or do not happen. Thus Gasking is right in insisting that  $p$  does not describe anything that happens in the world.

Nonetheless,  $p$  may be either true or false. It is true if it expresses

a rule which (it so happens) governs our arithmetical calculations. If, for any reason, we do not calculate according to that rule,  $p$  is false. And since, as Gaskin argues, it is conceivable that we relinquish the rule expressed by  $p$ , there is a conceivable state of affairs in which we would admit that  $p$  is false. It follows, on Gaskin's account, that ' $3 \times 4 = 12$ ' is a corrigible proposition.

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## ESCAPING THE GOOD SAMARITAN PARADOX

### I

WE shall examine two recent formulations of deontic logic, discussing issues we believe not yet satisfactorily resolved. Our discussion leads us to the formulation of a deontic logic adequate for the proof of the statements usually desired in such a system, in which the paradox of the Good Samaritan does not arise.

#### *The D-operator*

Prior<sup>1</sup> terms his axioms "a plausible set of postulates" for deontic logic, yet despite his enviable lucidity we find them difficult to understand. It is not clear whether the variables of the axioms stand for statements or actions. If for statements (in the proof of 27 Prior substitutes the statement COpp for a variable), then one wonders what it means to say a statement is obligatory, or forbidden. Or, if one is able to make some sense of this ("You shouldn't have said that, Coriolanus"), then it is plain that things other than statements may be obligatory. Apart from the substitution, Prior is committed to claiming that statements may be obligatory, for the first 'O' in his axiom O1 ranges over the statement COpp. Prior reads his axiom O1, OCOpp, as "It is obligatory that what is obligatory be done" or, expanding this, "It is obligatory that if p is obligatory then p be done (or, p is done)". Since actions, and not statements are done, Prior is therefore committed to substituting both actions and statements for the variables. But furthermore, is Prior's reading warranted by his notation? He reads the first 'p' as 'p', and the second as 'p is done', a shift which appears incompatible with the rigour Prior sets as his goal.

It seems to us desirable for clarity within the deontic calculus to have as obligatory either actions or statements, but not both. Since actions are intuitively the sort of things which may be obligatory, and statements, if at all, only in a derivative sense (the action of uttering the statement is obligatory, e.g. "You shouldn't have said that, Coriolanus"), we shall restrict the range of the deontic predicates to actions. We also introduce a new operator, the D-operator (axioms for which are presented in section II), which ranges over actions. 'Da' is to be read 'action a is done' or "action a is performed".

What then becomes of Prior's axioms? O1, OCOpp, is dropped for it either becomes COpOp and is not necessary as a special deontic axiom, or COpDp which is false. It is important to note that it cannot become COpODp, for here the second 'O' ranges over a statement, 'Dp'. Whatever plausibility this statement has can be

<sup>1</sup> "Escapism: The Logical Basis of Ethics" in Melden, *Essays in Moral Philosophy*

accounted for by pointing out that the malformed 'ODp', 'It is obligatory that p is done', if it means anything means 'Op', 'Action p is obligatory'. But then we have returned to the unnecessary 'COpOp'. Prior's O2, 'COpPp', remains intact, but O3, 'COCPqCOpOq', presents a slight difficulty. The first 'O' ranges over the statement Cpq, but as we have seen 'O' properly ranges only over actions. We may avoid this difficulty by interpreting 'Cpq' as 'ANpq', a complex action. Thus 'OCpq' is read 'The action not-p or q is obligatory'. (Prior, in reading O3, also reads 'Cpq' as an action and not a statement.) But now Prior's O4, 'CLCPqCOpOq', presents a problem, for if 'Cpq' is, despite appearances, a complex action and not a statement, then the modal operator 'L' ranges over the action Cpq, and it is not evident what this could mean. With the aid of the D-operator, however, the statement becomes clear. O4 is transformed into 'CLCDpDqCOpOq'.

### *The Paradox of the Good Samaritan*

We now turn to the paradox of the Good Samaritan, which can briefly be stated as follows. 'CLCDpDqFqFp', 'If 'p is done' strictly implies 'q is done' then if q is forbidden then p is forbidden', is a theorem of (the modification of) Prior's system, and consequently the following strange result is true as an instance of the theorem. 'If helping a robbery victim implies a person has been robbed then if the robbing is forbidden, helping the robbery victim is forbidden'. The Nowell-Smith Lemmon solution of the paradox<sup>1</sup> depends upon modifying the corresponding statement in the Andersonian system, 'CLCPqCLCqSLCpS', 'If p necessarily implies q then if q necessarily implies the sanction, then p necessarily implies the sanction'. They transform this statement into 'CLCKHxyRzyRzyCLCRzySzLCKHxyRzySz' which is read 'If x's helping y whom z robs necessarily implies that z robs y, then if z's robbing y necessarily implies that z is sanctionable then x's helping y whom z robs necessarily implies that z is sanctionable', and hence the paradox does not arise<sup>2</sup>.

<sup>1</sup> "Escapism The Logical Basis of Ethics", *MIND*, vol. LXIX, no. 275, July 1960.

<sup>2</sup> We should note that, as it stands, the Nowell-Smith Lemmon discussion of the paradox in the Andersonian system is inadequate. In their discussion  $\phi$  is a predicate in which x occurs free, such as 'x robs y'. 'Fx $\phi$ ' is defined as 'LC $\phi$ Sx', which is read (using the example to make the point clear) "'x robs y' strictly implies 'x is sanctionable'". But 'Fx $\phi$ ', we are told, is read 'x is forbidden to  $\phi$ '. Taking our example of 'x robs y' for  $\phi$ , we get the strange looking 'x is forbidden to x robs y', which we are hard put to understand. It may be that we have erred in considering  $\phi$  an open statement (which becomes a statement when a constant is substituted for x, or when x is bound by a quantifier), but if this is so, it becomes difficult to understand 'LC $\phi$ Sx'. For 'Sx' is read "x is sanctionable", and plainly is a statement (open statement), and one would think that only statements strictly imply statements, and therefore " $\phi$ " must be a statement (open statement).

However we should notice that an analogous solution does not suffice for Prior's non-Andersonian system. In this system the paradoxical system would become, modifying it along the lines of the Nowell-Smith-Lemmon solution 'CLCD<sub>xa</sub>DybCFybF<sub>xa</sub>' which we read, in a temporary notation which will not be used again, "If 'x does a' strictly implies 'y does b' then if y's doing b is forbidden, then x's doing a is forbidden". But this plausible-looking statement, it is easily seen, embodies the paradox. Hence we conclude that although Nowell-Smith and Lemmon may avoid the paradox in the Andersonian system, they have not got to its heart, for the solution they propose does not solve the analogous paradox deducible in Prior's system. But it is certainly important to solve the paradox in Prior's system since this system is not open to the objection Nowell-Smith and Lemmon so ably make to the Andersonian system, namely having modal operators range over moral statements.

### *The Solution of the Paradox*

The following is, we believe, one important key to the paradox. In the paradoxical statement CLCDpDqCFqFp as applied to the robbery case, it is not the action of helping the victim which entails he has been robbed, but the *description* of the victim which does so. This is easily seen in a case where the action has absolutely nothing to do with the entailment. 'If Macduff looks at a murderer then a murder has been committed, but murder is forbidden, so his looking at a murderer is forbidden'. Plainly in this case it is not the action of looking which entails the murder, but the description of the object of the action. In the robbery example, x's doing what he does, does not entail that a robbery has been committed, but 'x helps y and y has been robbed' does, and plainly here it is the statement 'y has been robbed' and not the statement about x which does the entailing.

To clarify the situation we introduce an I-operator, axioms for which are presented in section II, ranging over actions. 'Iab' is to be read 'action a is included in action b'. Although this is an undefined primitive operator, we hope the reading will make its meaning plain. As a further extra-systematic explanation we will say that 'Iab' is true if there exists a description of action b alone such that a description of action a is thereby given, where D is a description of b alone if D is a description of b, and no phrase of D can be eliminated without a less detailed description of b resulting. (We shall ignore descriptions in which sentences are repeated, and in which consequently some sentence can be eliminated without a less detailed description of b resulting.)

In the calculus presented in section II, the analogue of the paradoxical statement of Prior's system, 'CLCDpDqCFqFp', is 'CIabCFaFb', 'If action a is included in action b, then if a is



forbidden, b is forbidden'. Our claim is the following. The analogous statement in our system covers whatever cases lend Prior's paradoxical statement its plausibility, and excludes the paradoxical cases. For example, the samaritan paradox is no longer present, for robbing is not included in helping a robbery victim, as we have explained 'included'. It might also be thought that statements such as the following, which our theorem obviously does not cover, lend more plausibility to Prior's. If continued preparation for war implies a war will take place, then if war is forbidden, continued preparation for war is forbidden. But of course this statement is not covered by Prior's theorem either, for war is not *strictly* implied by preparation for it. This is not to assert that incorporating 'physically implies' or 'practically implies' into a deontic calculus would not be desirable. On the contrary. But it is not such a calculus we present here.

One final point before proceeding to the systematic development of the calculus. Someone might claim that for any action there are some circumstances under which it is justified, and consequently since no action is unreservedly forbidden, a deontic calculus can have no application. We might avoid this difficulty by saying that implicit in the 'F' is a context or situation which remains constant throughout a discussion, hence 'a is forbidden in such and such a context or situation'. (One who believes action a to be "absolutely" forbidden could state his position as follows: "a is forbidden in every context or situation".)

## II

1. Action Calculus, with action terms a, b, c, . . . , and axioms and rules isomorphic with those of the A-N or K-N sentential calculus; e.g. Sobocinski's K-N system with definition DA  $Aab = \text{df. } NKNaNb$ .

2. Sentential Calculus. Results obtained using the sentential calculus are labelled 'S.C.'.

3. Modal Calculus; e.g. T or S4.

4. D-I Calculus, employing operators D and I, which map action terms into statements.

### Axioms

- |                   |                     |
|-------------------|---------------------|
| I1. $EDNaNDa$     | I4. $CIAabcAIacIbc$ |
| I2. $CDaCIbaDb$   | I5. $IaKab$         |
| I3. $CIabCIbcIac$ | I6. $CINaNbIba$     |

### Rules

RI1 Uniform substitution of D and I statements for statements  
 RI2.  $AKabKNaNb \rightarrow CIacIbc$ ,  $CIcaIcb$ , where a, b, and c are syntactical action variables.

Definition:  $(a = b) = \text{df. } KLabIba$

5. Deontic Calculus, employing operator  $P$ , which maps actions into statements

### Axioms

- P1.  $CLDaPa$   
 P2.  $CIa bCPbPa$

### Rules

RP1 Uniform substitution of  $P$  statements for statements, provided that the statements substituted for are not bound or operated upon by 'L' or 'LC'.

RP2. Restriction on the application of the rule RL,  $p \rightarrow Lp$ , to exclude the form  $LPa$

### Definitions

- DP1  $Oa = df \ NPNa$   
 DP2.  $Fa = df. NPa$

### Some Sample Theorems

- T1.  $CIbaCDaDb \ I2, S.C.$   
 T2  $C(a = b) EDaDb$   
     1.  $OKCpqCrCKprKqs \ S.C.$   
     2.  $CKIbaIabKCDaDbCDbDa \ 1, p/Iba, q/CDaDb, r/Iab, s/CDbDa, T1$   
 T3.  $C(a = b) EPaPb$  Similarly, with P2 replacing T1, P replacing D  
 T4  $CIKabcKIabc$   
     1.  $CIKabcIac \ I3, b/Kab, I5$   
     2.  $CIKabcIKbac \ RI2$   
     3  $CIKabcIbc \ 1, a/b, b/a, \text{result by } 2, S.C.$   
 T5.  $IAaba$   
     1.  $INaNNKNaNb \ I5, a/Na, b/Nb, RI2$   
     2  $IAaba \ 1, DA, I6, S.C.$   
 T6  $CAIacIbcIAabc$   
     1.  $CIacIAabc \ I3, a/Aab, b/a, T5$   
     2  $CIbcIAabc \ I3, a/Aab, T5, \text{result by } 1, S.C.$   
 T7  $Iaa \ I5, b/a, RI2$   
 T8.  $IaAaa$   
     1.  $CINaNaINNKNaNaN a \ RI1$   
     2.  $INaAaaNa \ T7, 1, DA, \text{result by } I6$   
 T9  $CIbaINaNa \ I6, a/Nb, b/Na, RI2$   
 T10.  $CIaAbcAIabIac$   
     1.  $CINaNKbcIKbca \ I6, b/Kbc$   
     2.  $CINaNKbcKIbaIca \ 1, T4, S.C.$   
     3.  $CINN aNKbNbNcKINbNaINcNa \ a/Na, b/Nb, c/Nc$   
     4.  $CIaAbcKIabIac \ DA, I6, \text{result by } S.C.$

T11. CADaDbDAab

1. AIAabaIAabb T10, a/Aab, b/a, c/b, T7

2. ACDAaDAabCDbDAab 1, T1, S C, result by S C

T12. CAPaPbPAab similarly, replacing D by P in T11, and using P2.

T13. CDAabADaDb

1. AIAaAbIAaAb I4, c/Aab, T7

2. ACDAaDbDAaCDaDb T1, S C, 1, result by S.C.

T14. CPAabAPaPb similarly, using P2

T15. CLiabCObPa

1. CLiabLCDbDa T1, Fey's T, RL

2. CLiabLDANba 1, S.C., II

3. CLiabPANba 2, S.C., P1, result by T12, T14, S C, DP1

T16. COaPa

1. CLDAaNaPAaNa P1, a/AaNa

2. ADaNDa S.C., p/Da

3. LDAaNa 2, RL, II, T11

4. APaPNa 1, 3, T14

5. CNPNaPa 4, S C, result by DP1

T17. COaMDa

1. CLDNaPNa P1, a/Na

2. CNPNaNLNDa 1, S.C., II, result by DP1 and T

T18. CKOaNPKaNbOb

1. IANaNBnNb I5, S.C.

2. IANaKaNBnNb 1, RI2

3. CPNbPANaKaNb 2, P2

4. CPNbAPNaPKaNb 3, T14

5. CKNPNaNPKaNbNPNb 4, S C, result by DP1

T19. ClabCFaFb P2, S.C., DP2

Note that 'CLDaOa', which is derivable in (the modification of) Prior's system, is not derivable in this system.

Since this was written we have discovered some counter examples to our explanation of one action's being included in another. The simplest one is the case where the time at which action b is performed is specified by saying 'b occurs two hours after —', where the blank is filled in by a detailed description of some other action. Consequently the explanation given in the text requires modification.

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## BOOLEAN ALGEBRA AND THE PROPOSITIONAL CALCULUS

MANUALS of logic all comment on the kinship between Boolean Algebra (BA) and the propositional Calculus (PC). Some mention, in particular, that the propositional rewrite of any theorem of BA, that is, the result of writing ' $\sim$ ' for ' $-$ ', ' $\vee$ ' for ' $\cup$ ', and ' $\equiv$ ' for ' $=$ ', for example, in any theorem of BA, is a tautology and hence a theorem of PC. None, however, shows how to turn the propositional rewrites of standard axioms (or axiom schemes) and rules of inference for BA, E. V. Huntington's fourth set, for example, into a complete set of axioms (or axiom schemes) and rules of inference for PC.<sup>1</sup> Instructions to that effect are offered here.

(1) Let BA be cast in the following form. (a) The primitive signs of BA are to be a denumerably infinite list of (class) variables, the two operators ' $-$ ' and ' $\cup$ ', the predicate ' $=$ ', and the two parentheses '(' and ')', (b) The terms of BA are to be all variables, all expressions of the form  $\bar{\alpha}$ , where  $\alpha$  is a term of BA, and all expressions of the form  $(\alpha \cup \beta)$ , where  $\alpha$  and  $\beta$  are terms of BA, (c) The sentences of BA are to be all expressions of the form  $\alpha = \beta$ , where  $\alpha$  and  $\beta$  are terms of BA, (d)  $(\alpha \cap \beta)$  is to be short for  $(\bar{\alpha} \cup \bar{\beta})$ , (e) The axioms of BA are to be all sentences of BA of any one of the following four forms

$$\begin{aligned} \text{BA1} \quad & (\alpha \cup \beta) = (\beta \cup \alpha) \\ \text{BA2} \cdot & ((\alpha \cup \beta) \cup \gamma) = (\alpha \cup (\beta \cup \gamma)) \\ \text{BA3} \cdot & ((\alpha \cap \beta) \cup (\alpha \cap \bar{\beta})) = \alpha \\ \text{BA4} \quad & \alpha = \alpha \end{aligned}$$

(f) The rules of inference of BA are to be

BA5 From  $\alpha = \beta$  to infer  $\beta = \alpha$

BA6 From  $\alpha = \beta$  and  $\beta = \gamma$  to infer  $\alpha = \gamma$

BA7 From  $\alpha = \alpha'$  to infer  $\beta = \beta'$ , where  $\beta'$  is like  $\beta$  except for containing occurrences of  $\alpha'$  at one or more places where  $\beta$  contains occurrences of  $\alpha$ .

*Note* As the reader undoubtedly knows, BA7 may be replaced by three of its own subcases, to wit:

$$\begin{aligned} \text{BA7.1} \quad & \text{From } \alpha = \beta \text{ to infer } \bar{\alpha} = \bar{\beta} \\ \text{BA7.2} \quad & \text{From } \alpha = \beta \text{ to infer } (\alpha \cup \gamma) = (\beta \cup \gamma) \\ \text{BA7.3} \quad & \text{From } \alpha = \beta \text{ to infer } (\gamma \cup \alpha) = (\gamma \cup \beta). \end{aligned}$$

A similar remark applies, *mutatis mutandis*, to PC7 below.

(2) Let PC be cast in the following form. (a) The primitive signs of PC are to be a denumerably infinite list of (propositional) variables, the two connectives ' $\sim$ ' and ' $\vee$ ', and the two parentheses

<sup>1</sup> See E. V. Huntington, *Trans. Amer. Math. Soc.*, xxxv (1933), 274-304.

'(' and ')', (b) The sentences of PC are to be all variables, all expressions of the form  $\sim A$ , where  $A$  is a sentence of PC, and all expressions of the form  $(A \vee B)$ , where  $A$  and  $B$  are sentences of PC, (c)  $(A \& B)$  is to be short for  $\sim(\sim A \vee \sim B)$ ,  $(A \supset B)$  short for  $(\sim A \vee B)$ , and  $(A \equiv B)$  short for  $((A \supset B) \& (B \supset A))$ ,<sup>1</sup> (d) The axioms of PC are to be all sentences of any one of the following four forms

$$\text{PC1. } (A \vee B) \equiv (B \vee A)$$

$$\text{PC2: } ((A \vee B) \vee C) \equiv (A \vee (B \vee C))$$

$$\text{PC3: } ((A \& B) \vee (A \& \sim B)) \equiv A$$

$$\text{PC4: } A \equiv A$$

(e) The rules of inference of PC are to be, *for the time being*.

PC5 From  $A \equiv B$  to infer  $B \equiv A$

PC6 From  $A \equiv B$  and  $B \equiv C$  to infer  $A \equiv C$

PC7 From  $A \equiv A'$  to infer  $B \equiv B'$ , where  $B'$  is like  $B$  except for containing occurrences of  $A'$  at one or more places where  $B$  contains occurrences of  $A$

(3) PC1-PC7, the propositional rewrites of BA1-BA7, only yield tautologies of the form  $A \equiv B$  as theorems.<sup>2</sup> PC1-PC7 *plus* the following rule of inference

PC8 From  $A$  and  $A \equiv B$  to infer  $B$

constitute, however, a complete set of axiom schemes and rules of inference for PC. Proof is as follows.

Note first that

$$(A \equiv A) \equiv ((A \vee A) \supset A)$$

$$(A \equiv A) \equiv (A \supset (A \vee B))$$

$$(A \equiv A) \equiv ((A \supset B) \supset ((C \vee A) \supset (B \vee C)))$$

are short for propositional rewrites of theorems of BA and hence are theorems of PC. But by PC4  $A \equiv A$  is a theorem of PC. Hence by PC8

$$(A \vee A) \supset A \tag{1}$$

$$A \supset (A \vee B) \tag{2}$$

$$(A \supset B) \supset ((C \vee A) \supset (B \vee C)) \tag{3}$$

are theorems of PC (Step one). Note also that *Modus Ponens*, to wit.

If  $A$  and  $A \supset B$  are theorems of PC, then  $B$  is a theorem of PC, is available here as a derived rule of inference. Let indeed  $A$  and  $A \supset B$  be theorems of PC.  $(A \supset B) \equiv (A \equiv (A \& B))$  is short for a propositional rewrite of a theorem of BA and hence is a theorem of PC

<sup>1</sup> From now on a few easily restored parentheses are omitted

<sup>2</sup> The point was brought to my attention by Professor R. McNaughton.

Hence by PC8  $A \equiv (A \& B)$  is a theorem of PC and hence by PC8 again  $A \& B$  is a theorem of PC. But  $A \equiv (A \equiv (A \equiv A))$  is short for a propositional rewrite of a theorem of BA and hence is a theorem of PC. Hence by PC8  $A \equiv (A \equiv A)$  is a theorem of PC and hence by PC7  $(A \equiv A) \& B$  is a theorem of PC. But  $((A \equiv A) \& B) \equiv B$  is short for a propositional rewrite of a theorem of BA and hence is a theorem of PC. Hence by PC8  $B$  is a theorem of PC (Step two). Note finally that (1)-(3) and *Modus Ponens* yield all tautologies as theorems (Step three).<sup>1</sup> By steps one, two, and three PC1-PC8 must therefore constitute a complete set of axiom schemes and rules of inference for PC. Q. E. D.

*Note.* In the first draft of a paper submitted to *The Journal of Symbolic Logic* in the summer of 1958 I remarked that PC1-PC7, *Modus Ponens*, and the following rule of inference

PC10 From  $(A \vee B) \equiv B$  to infer  $A \supset B$ ,

constitute a complete set of axiom schemes and rules of inference for PC. A few months later I learned from a referee for the same journal that PC8 may substitute for PC10.<sup>2</sup> Neither one of us, however, realized at the time that *Modus Ponens* is redundant in the presence of PC8 and hence may be dispensed with. In the summer of 1960 Professor J. Porte finally informed me that in view of a theorem of A. Tarski's PC1-PC7 plus any one of PC8, PC11, PC12, and PC13, where the latter rules of inference respectively read.

PC11 From  $A \equiv B$ , where  $B$  is any one of PC1-PC3, to infer  $A$   
 PC12 From  $A \equiv (B \equiv B)$ , where  $B$  is any sentence of PC, to infer  $A$   
 PC13 from  $A \equiv (A \equiv A)$  to infer  $A$

should constitute a complete set of axiom schemes and rules of inference for PC.<sup>3</sup> The proof offered here that PC1-PC8 constitute such a set is believed to be new.

(4) The above set of axiom schemes and rules of inference may be simplified in two ways.

First, PC5 and PC6 are redundant in the presence of PC8 and hence may be dispensed with. Note indeed that by PC1  $(\sim(A \supset B) \vee \sim(B \supset A)) \equiv (\sim(B \supset A) \vee \sim(A \supset B))$  is a theorem of PC and hence by PC7  $(A \equiv B) \equiv (B \equiv A)$  is a theorem of PC. Hence if  $A \equiv B$  is a theorem of PC, then by PC8  $B \equiv A$  is a theorem of PC. Note also that if  $A \equiv B$  is a theorem of PC, then by the same reasoning  $B \equiv A$  is a theorem of PC, and hence by PC7  $(B \equiv C) \equiv (A \equiv C)$

<sup>1</sup> See E. Göthelid in *Norsk. Matematisk Tidsskrift*, xxix (1947), 1-4, and H. Rasowa, *ibid.* xxxi (1949), 1-3.

<sup>2</sup> Both results are announced in a forthcoming issue of *The Journal of Symbolic Logic*.

<sup>3</sup> The theorem was cited by Tarski in the course of lectures given in Paris in 1955 and was dubbed by him "the completeness theorem of the word algebra".

is a theorem of PC. Hence if  $A \equiv B$  and  $B \equiv C$  are theorems of PC, then by PC8  $A \equiv C$  is a theorem of PC.

Second, PC7 and PC8 may be replaced by a single rule of inference, namely

PC9 · From  $A \equiv A'$  and  $B$  to infer  $B'$ , where  $B'$  is like  $B$  except for containing occurrences of  $A'$  at one or more places where  $B$  contains occurrences of  $A$ .

That PC7 and PC8, on one hand, yield PC9 can be seen as follows. If  $A \equiv A'$  is a theorem of PC, then by PC7  $B \equiv B'$  is a theorem of PC. But if  $B$  and  $B \equiv B'$  are theorems of PC, then by PC8  $B'$  is a theorem of PC. Hence if  $A \equiv A'$  and  $B$  are theorems of PC, then  $B'$  is a theorem of PC. That PC9, on the other hand, yields PC7 and PC8 can be seen as follows. PC8 is a mere subcase of PC9 with  $B$  for  $A'$ ,  $A$  for  $B$ , and  $B$  for  $B'$ . As for PC7, by PC4  $B \equiv B$  is a theorem of PC. But  $B \equiv B'$  is like  $B \equiv B$  except for containing occurrences of  $A'$  at one or more places where  $B \equiv B$  contains occurrences of  $A$ . Hence if  $A \equiv A'$  is a theorem of PC, then by PC9  $B \equiv B'$  is a theorem of PC.

PC1-PC4, PC7, and PC8, on one hand, PC1-PC4 and PC9, on the other, thus constitute complete sets of axiom schemes and rules of inference for PC.

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## TRUTH AS APPRAISAL

ONE of the most recent of the many theories of truth advanced by philosophers at various times is the Appraisal or Evaluative Theory of Truth, which was advanced by Mr. A. R. White a few years ago in *MIND* ("Truth as Appraisal", lxi (July, 1957), 318-330). This theory has been ably, and to my mind effectively, refuted by Mr. Bernard Mayo in another article in *MIND* ("Truth as Appraisal", lxxviii (January, 1959), 80-86). I fully agree with Mr. Mayo that there is no appraisive aspect of truth, *in the sense of 'true' (and 'false') in which it is normally applied to statements, opinions, beliefs or propositions*. I also think that Mr. Mayo has proved this by showing "(1) that 'true' and 'false' are polar opposites and do not admit of degrees, and (2) that even if a case could be made out for non-scalar evaluations, yet it has not been shown that any 'pro-attitudes' are involved in standard contexts of 'true'" (*ibid.* p. 80). I shall therefore not attempt to go over the same ground again, but shall content myself with making some observations, not made by Mr. Mayo, by way of reinforcing his argument and his general conclusion as regards the use of 'true' and 'false' in the sense already mentioned.

### I

White, in explaining what he means by saying that "the function of the word 'true' (or 'false') is to *appraise* whatever it is used of, whether statements or objects" (*op. cit.* p. 318) states that 'true' "praises, directs, guides, encourages, agrees, endorses, expresses a favourable attitude, sets up standards" (p. 330). On the other hand, Mayo holds that "in fact it does none of these things, except incidentally and in conjunction with other factors which need to be made explicit . . ." (p. 82). Mayo, however, makes no direct attempt to show this, except in respect to White's claim that 'true' expresses a favourable (a "pro-") attitude when used in relation to statements, opinions, beliefs or propositions. I therefore propose to begin by attempting to make up this deficiency.

To begin with, White surprisingly does not tell us *what* it is that 'true' is supposed to praise, direct, guide, encourage, and so on. Now there are only four things which could conceivably constitute the objects of one or more of these attitudes or activities in relation to statements. The same applies, *mutatis mutandis*, to beliefs, opinions, etc.: (a) the person P affirming the statement S said to be true (or false), (b) the object(s) referred to by S, (c) what S itself "says" or *expresses*, the beliefs or opinions it conveys. B, and (d) S's *truth*, if it is true, T. In the case of all the presumed evaluative functions except *endorsing*, what is appraised can only be P or T or S. Thus if P says, "That is a carnation", and I reply: "Yes what you say is true", or, "Your statement is true", S<sub>1</sub>. I will certainly be agreeing with him and with S; but I do not see that I will necessarily be praising, directing, guiding, encouraging,



expressing a favourable attitude toward, or setting up standards for him. On the other hand,  $S_1$  expresses my endorsement, but my endorsement not of P or T (it does not make sense to speak that way) but of the statement S. And the only thing which I could conceivably be doing in respect to O, here the carnation, in making  $S_1$  would be praising, expressing a "pro-attitude" toward, it. Actually however, as Mayo points out in relation to "That is a true geranium", I would certainly not be praising or expressing a "pro-attitude" toward O, toward the flower itself. If I wanted to do so, I might add, I would not make  $S_1$ , or any similar statement, but rather some such statement as "That is a lovely carnation!" ( $S_2$ ).

No doubt it is possible, with some ingenuity, to think of situations in which, by making  $S_1$ , I would be, among other things, praising, guiding, encouraging, expressing a "pro-attitude" toward or setting up standards for P. Thus if P is a pupil of mine, and he correctly identifies a given flower as a carnation, I *may* intend to convey praise or a "pro-attitude" for his knowledge, smartness, etc., when I make  $S_1$ . On the other hand, I *may* intend to guide or encourage my rather dull pupil's painful attempts to identify a simple flower when after long thought or fumbling he blunts out "That is a carnation!" In both types of cases, however, it is not my use of the word 'true' that would express or convey all this—that is why I said that the foregoing *may* or *may not* be the case—but rather the tone of my voice, the smile on my face or the pat on his shoulder accompanying my utterance of  $S_1$ . In every case 'true' itself would be evaluatively non-committal toward P. As for my endorsement of S by means of  $S_1$ , that, in itself, does not mean or imply my having a "pro-" or any other kind of attitude toward S or even toward S's truth. If S were a flattering statement about myself, however, I would very likely, though not invariably, since I may detect or imagine ulterior motives in P's uttering S, or regard P as too inferior to me for me to be happy at his compliments, acquire a "pro-attitude" toward P as a result of his making S. I would also, almost certainly, look favourably upon the opinion which S expresses about me. But this attitude would be a result of my having a "pro-attitude" toward myself, *not* O. If P's flattering statement had been about my enemy, I would have had the exact opposite of a "pro-attitude" toward P and his opinion! The "pro-attitude" toward P and his opinion is only transferred from a like attitude toward O. Further, it is not my endorsement itself, my saying that S is true, that expresses this attitude towards these—it is again the tone of voice with which I utter  $S_1$ , or the smile, etc., that accompanies it. Once more, 'true' itself is evaluatively non-committal. It is precisely because 'true' is evaluatively non-committal that I can have a "con-attitude" toward P even as I, grudgingly, endorse P's flattering statement about my enemy!

White also holds, we will recall, that 'true' sets up standards, by which I presume he means that the person using 'true' in a

statement of the form "S is true" would be setting up standards of truth and falsity for the person who had made S, in using or applying 'true' to it. But now, does this mean that "S is true" itself means (1) 'There is a standard of truth to which S conforms'? If this is what White has in mind, it is simply not true. What "S is true" does mean or at least part of what it means, is (1) "There is something (whose nature is left unspecified) which characterizes S, and which is what we normally call truth". Here there is no explicit reference to standards. However, the statement "S is true" logically implies, but it does not entail, that there exists a criterion or standard of truth, which is satisfied in S's case. Further, it is often the case that when we judge S to be true we *appeal* to or use a criterion or standard of truth: but "S is true" does not itself express this. Also, one's using or appealing to a standard is somewhat different from one's setting up a standard: a standard that is used may but it also may not, be set up by its user.

I said that "S is true" logically implies that there exists a criterion or standard of truth, which is satisfied in S's case. I should now state that this is different from, and does not entail or even imply, that whenever anybody makes a statement of the form "S is true" he would necessarily have in mind, or think he is in possession of, a criterion or standard of truth, with whose help he discovers that S is true. The foregoing can be legitimately inferred, in any particular instance of the framing of "S is true" only under what I shall call "standard conditions" of the application of 'true' and 'false'. For instance, no such inference can be legitimately made where a child nods his assent to his father's statements, or keeps repeating, "That is true, dad", every time his father affirms anything, out of sheer fear of his parent. In similar fashion we would rightly not infer that a person X who says that "S is true" believes that S is true, if we know that he is joking, is speaking ironically, is talking in his sleep or under anaesthesia, and so on.

The foregoing discussion of the alleged appraisive function of 'true' and 'false' as applied to statements has an important consequence in relation to another part of Mr White's thesis. We have seen that, even if White's main thesis is correct, the different alleged appraisive functions of 'true' relate to different kinds of things involved in asserctions of the form "S is true", sometimes, depending on attendant circumstances. We have also seen that in the case of no one instance of "S is true" are *all* the alleged appraisive functions simultaneously present. From this we can validly infer, it seems to me, that at least some, and perhaps even all, of these functions cannot be regarded as giving us the *meaning* of 'true'. For it can be readily shown, though I shall not attempt it here, that in its relevant ordinary senses as it is applied to verbal expressions, meaning is something relatively constant or fixed. If this is so, White is wrong in holding that all these evaluative functions, which, according to him "remain invariant", give us the meaning of 'true'.

while the "descriptive (function of 'true'), which may be said to give the *criteria* for saying that anything is true, varies with each kind of statement. . ." (*op. cit.* p. 318). The only invariant functions of the use of 'true' on White's list are agreement and endorsement. The "setting up of standards," even if this is understood as grading in a non-scalar sense of this term, is not, as we have seen, an invariant function of 'true'; even as something implied and not expressed by the ordinary applications of this word to statements.<sup>1</sup>

## II

Mayo has shown, and I shall not repeat his arguments, that 'true' and 'false' are polar opposites; that truth itself, *contra* White, cannot be appraised in comparative, in the sense of *scalar*, terms, but that, "when a statement has been appraised as false, then another, scalar criterion may be put to work, yielding a new range of appraisals expressible in comparative terms like 'nearer to/further from the truth' (p. 85)." From the fact that scalar terms cannot be employed in relation to 'truth', Mayo validly infers that 'true' does not have an appraisive function, since the possibility of employing scalar terms in relation to truth is necessary for 'true' to possess an appraisive function in White's sense. He also validly infers, though only by implication, that 'false' is also a non-appraisive expression: it being implicitly assumed, and rightly so I think, that 'true' could not be non-appraisive without its opposite, 'false', being non-appraisive too. This, despite the fact that Mayo has shown, and White too for that matter, that scalar terms can be employed in relation to falsity. From these things we can conclude that the possibility of a scalar use of an expression is not a *sufficient* condition of its being an appraisive expression in White's sense; in the sense in which 'good', say, is appraisively used at least in its ethical and aesthetic senses, *i.e.* in an axiological sense.

Although our last proposition is a conclusion *derivable* from Mayo's arguments, it can be demonstrated on independent grounds. If that is done, we would clearly have new grounds in support of Mayo's and our contention that White has failed to prove his main thesis. For if the proposition under consideration is shown to be true, it will follow that White, even if he were successful in showing that truth, in addition to falsity, admits of scalar grading, has not thereby succeeded in showing that 'true' and 'false' can be properly used to grade anything in an axiological sense; which is what he sets out to prove, since he constantly compares these expressions with 'good' as an appraisive expression.

<sup>1</sup> Throughout the present paper I am assuming with White himself for the sake of argument, that the functions he attributes to 'true' and labels "evaluative" or "appraisive" are *all* genuinely so in the relevant ordinary sense of these words, or in some important philosophical sense, which, however, White does not state in his paper. Whether or not this is true is an important question, but I am not concerned with it in my discussion.

Our proposition can be demonstrated rather quickly and without much difficulty by reflecting that there are many expressions, normally used in a scalar manner, which yet do not grade in an axiological sense. A few examples are 'hot', 'cold', 'light', 'dark', as applied to colours or shades of colours, 'wide', 'narrow', 'bright', 'dull' (of colours and of persons and animals), 'emotional', 'unemotional', 'short', 'tall', 'slim', 'fat'. Some of these expressions, for example 'slim', 'fat', 'short', 'tall', do possess an emotive colouring. but they also can be and are used in a purely neutral way, *e.g.* in science, while others for example 'light' and 'dark' as applied to colours or objects (not to human beings, though!), and 'wide' and 'narrow', do not possess an emotive colouring under *most* actual conditions. But of course there are practically no adjectives, nouns, verbs or adverbs—words that refer to, are about, something—that under special circumstances cannot acquire some emotive colouring. Even in the case of the pairs of expressions that generally have an emotive colouring, the expression of a "pro-" or a "con-attitude" is not a function which they possess by convention; but rather is acquired along the way by association with the objects to which they refer. Whereas expressions that grade in an axiological sense, such as 'good' and 'bad', at least in their most important uses, are intended by convention to express a "pro-" or a "con-attitude" as part of their normal function *qua* verbal expressions.

It is not difficult to see that there are interesting differences between the manner in which expressions such as 'light' and 'dark', 'tall' and 'short' function and the manner in which 'true' and 'false' function. For one thing, the former pairs of expressions are not polar opposites, unlike 'true' and 'false'; and *both* expressions, in any one pair, can be properly employed to grade in a scalar sense, and in terms of one and the same scale. Whereas, as we have mentioned before, 'true' cannot be employed in that manner. A person may be fatter or slimmer, lighter or darker, more emotional or less emotional or less unemotional than another; whereas a statement cannot, in a literal sense, be truer or less true, though it can be less false or more false, than another. It remains, nevertheless, that the former expressions can be properly employed to grade in a scalar sense; though not axiologically. And that was what we were interested in establishing. At the same time, the fact, if Mayo's and our contention is true, that falsity admits of scalar gradations though it is non-appraisive in White's sense shows that in important respects 'false' is on all fours with expressions such as 'slim', 'fat', 'bright' and 'dull'. From this it follows that 'true' too, though different from these last expressions in the important respect mentioned above, is similar to them in possessing a grading function in the sense explained, in a non-axiological sense.

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## A NOTE ON HARRISON'S "SOME USES OF 'GOOD' IN CRITICISM"

HARRISON wants to show that critics do not deal with a special kind of recognition, i.e. it is in vain to search for exactly what critics recognize. Harrison means that it is not the case that the good critic sees some special quality "the essential nature of art" in the piece of art in question, something which the layman does not see of which the noticing would put the layman in the same situation as the critic and allow him to state the same judgment.

According to Harrison competent critics are able to predict whether a picture, for instance, is going to be a failure or not. But the case that critics say a picture is bad in a certain extent makes the picture a failure, this does not necessarily mean that the picture has to be bad or, as Harrison holds, that the failure implies the picture is bad.

If a picture is accepted for an exhibition and there gets a very good critique and is then purchased by a museum where it will be displayed and viewed by visitors, you can well say the picture was a success. The question whether the picture is good still remains. But suppose instead this picture was never accepted for an exhibition because the critics for the gallery found it unworthy, in this case it will not be placed in a museum but will be closeted and never receive its chance to be revalued. The picture then is a failure but in no way is it possible to declare that the picture is bad.

This reasoning is, of course, too categorical and therefore, in a certain sense, wrong but all the same enlightening because the fact remains what critics think and say about a piece of art *does* play an important role and makes the work a failure or success, not good or bad.

Another question arises. How does it happen that competent critics often do come to the same conclusion? Has that not to do with the work outside critics themselves and their world of language?

If all persons in a room see that a window is broken it must be the case, generally at least, that it depends on some quality of the window, namely, that it is broken.

This kind of a decision, whether a work is a failure or not, is according to Harrison only the first step. He calls this judgment an acceptive sort of good which simply means that the work is not trivial but it is worth further examination or discussion.

The most important phase in criticism comes, according to Harrison, after this first step and the second step is to state the piece of art's special merits and the third to place it in the art history, literature, etc., giving the piece of art's relationship to other works of art.

Harrison calls step one "simply a preliminary skirmish" meaning something less important for critics which they do not have to spend so much time on. For me however, step one seems very important as it decides whether the critic is going to bother employing steps two and three on the piece of art.

If we accept a two-value logic saying that if a quality is not bad it is good and *vice versa*, then we must say that a book which is not a failure is in some respect a success and in this way you could from Harrison's negative art theory construct a positive art theory. Besides, it is hard to see why it is more difficult to construct a positive art theory than a negative one, if it is possible, as Harrison seems to believe, to create a negative art theory and give, on the basis of experience, the qualities which make a book a failure, then why should it not be possible, on the same material of experience, to name some qualities that make it probable that the book is going to be a success?

The consequence of Harrison's definition of acceptive good would be that when one in daily life says that *x* is better than *y* it would follow that one ought to spend more time on *x* or consider *x* more than *y*. But Harrison does not draw this conclusion, instead he says that here is a question of another good which he calls the directive good, a kind of good that has to do with a decision from the majority of good critics.

Harrison seems to mean that the quantity decides the quality. The more good critics who think *x* is good, the better *x* is. Harrison does not give the definition of a good critic and as long as we do not know which people are good critics it seems we do not know what art is good or bad.

The expression "*x* is better than *y*" meaning "*x* is generally said to be better than *y*" or "people who know something about this matter hold *x* to be better than *y*" does not seem to have much to do with art. Assume that one takes two marbles which are identical in all secondary qualities and names, at random, one of them *A* and the other *B*. He then asks ten good critics which one they think is aesthetically the better of the two. Assume now that all the critics say *A* is the better, then according to Harrison, *A* is better because this is the good critics' choice. But one has in no way explained the meaning of "better" by that because what we are interested in is to know *why* the good critics said *A* was better than *B*, we are interested in their use of "good" and "better."

The good critics speak the same language as the layman. In deciding the use and meaning of a language one must not go to an echo to find out how it uses the words or what it means by the words it says because in this way one will get nowhere.

The absurdity of Harrison's reasoning about directive good can be illustrated in a simple example. *X* is going to be judged by the critics P1-P8. Assuming that we know P1's-P8's reactions to art pieces similar to *X*, we can then predict how they are going to value *X*. Let us say that *X* is going to be a success. If instead P9-P16, with the contrary opinion, take the other critics' places and evaluate *X* then *X* would become a failure. That means *X* is both hypothetically good and bad, which is a contradiction.

Harrison's theory of the majority decision of course has its

weakness in cases when you do not get a majority for one alternative. But even when you get a majority for a positive or negative evaluation, how is it possible to justify that X which is valued is really good because the majority says so?

A leader of a caravan who has an hallucination of an oasis in the desert is not supposed to give a correct judgment when he says he sees an oasis by the simple fact that there is no oasis, and the caravan leader's judgment is in no way going to be more correct because all the camel drivers have the same hallucination and confirm the chief's opinion.

It is interesting to read Harrison's examples which he says confirm his theories. He states that it is reasonable to accept his ideas because "it seems that people are sufficiently alike for the critical development of different individuals to follow a fairly regular pattern. I mean by this, for example, that one goes from enjoying 'Swan Lake' via 'Eine Kleine Nachtmusik' . . . and not vice versa." We can very well go the opposite way and there is not something very surprising in this as Harrison believes. All Harrison's examples are open to the same criticism.

Harrison takes up a third meaning of good called appraisive good. When a critic says "x is good" in the sense of appraisive good he simply means "x has the definite characteristics K<sub>1</sub>-K<sub>n</sub>". Here we have arrived at what Moore called the "naturalistic fallacy". "Good" in "x is good" cannot be defined by replacing "good" by a number of names of other qualities.

All the three kinds of good Harrison tries to state are in my opinion not so practical and it is very questionable that it is the case that they describe the essential characteristics of the uses of "good". I think, in describing the meaning and use of the term "good" we have, at the same time, to regard the qualities of the work, those of the spectator, the situation in which the work and spectator are, and the relationships between spectator and work. This is a task much more difficult than the one Harrison took himself but will probably give a much better result.

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## VII.—CRITICAL NOTICES

*The Concept of Law.* By H. L. A. HART Clarendon Press.  
Oxford University Press 1961. 21s

### I

MODERN British philosophy has long lacked any systematic treatment of problems about law that would stand comparison with the fundamental theories of other times or places—with the philosophies of Savigny, Austin, Kelsen and others. Professor Hart has now remedied this lack with a book which has all the virtues of elegance and clarity that characterise British philosophical writing at its best. If, like any work of first-rate philosophical importance, the book forces one to re-think one's fundamental conceptions of its subject-matter, its value in this respect is not depreciated when one comes to see that not all of its arguments are conclusive and not all of its assertions are irrefutable.

Much of the book is taken up with a powerful critique of other juristic theories, and especially of various positivist and natural law doctrines. But the book's peculiar interest undoubtedly lies in Hart's own theory, which gradually emerges from his criticisms of Austin. Hart's thesis is that the difference between rules which impose obligations, or duties, and rules which confer powers, is of crucial importance in jurisprudence (237). Rules of the former kind he calls primary, and of the latter kind, secondary. Under rules of the primary type human beings are required to do or abstain from certain actions, whether they wish to or not. Rules of the other type are in a sense parasitic upon or secondary to the first, for they provide that human beings may by doing or saying certain things, introduce new rules of the primary type, extinguish or modify old ones, or in various ways determine their incidence or control their operations. Primary rules impose duties; secondary ones confer powers, public or private. Primary rules concern actions involving physical movement or changes; secondary rules provide for operations which lead not merely to physical movement or change, but to the creation or variation of duties or obligations (78, 79). Thus criminal law, apart from its provisions for sanctions, consists of primary rules (27, 28, 31, 32). But the legal rules defining the ways in which valid contracts or wills or marriages are made do not require persons to act in certain ways whether they wish to or not. Instead they provide individuals with facilities for realising their wishes, by conferring legal powers upon them to create, by certain specified procedures and subject to certain conditions, structures of rights and duties within the coercive framework of the law (27). Similarly it is secondary rules that lie behind the exercise of legislative and judicial powers (31).

Hart claims that law can best be understood as a union of these two diverse types of rule (237). He admits, of course, that many



other distinctions could and, for some purposes, should be drawn. But he insists that in the combination of these two types of rule there lies what Austin wrongly claimed to have found in the notion of coercive orders, namely, 'the key to the science of jurisprudence' (79). Again, his view is not that wherever the word 'law' is properly used this combination of primary and secondary rules is to be found, for he thinks it clear that the diverse range of cases of which the word 'law' is used are not likened by any such simple uniformity, but by less direct relations—often of analogy of either form or content to a central case (79). But the union of primary and secondary rules is at the centre of a legal system, even though it is not the whole and as we move away from the centre we have to accommodate elements of a different character (96), and the reason why Hart accords this union of elements a central place is because of their explanatory power in elucidating the concepts that constitute the framework of legal thought (79). Most of the features of law which have proved most perplexing and have both provoked and eluded the search for definition can best be rendered clear if these two types of rule and the interplay between them are understood (79).

Unfortunately, however, the large claims that Hart makes on behalf of his distinction between primary and secondary rules do not seem altogether justifiable. The distinction turns out to be scarcely capable of playing the role for which Hart casts it.

## II

This begins to become clear when we examine the extent to which it is possible to class most legal rules as primary or secondary in Hart's sense. Secondary rules, we are told, confer powers, whether public or private. But has the concept of power quite so wide an extension as Hart assumes? No doubt usage is no more uniform here than elsewhere, but there does seem something a bit odd in talking about 'the power . . . conferred on individuals to mould their legal relations with others by contracts, wills, marriages, etc' (28). Normally, legally recognised powers to act in certain specified ways are conferred only on certain specified persons or categories of persons. One named person may be given power of attorney over the estate of another or the power to execute his will, trustees may be given powers to buy and sell stock, Chief Constables have the power (discretion) to determine whether or not a motorist is to be publicly prosecuted for a parking offence, the Colonial Development Corporation has been given power to investigate and formulate development projects, and a County Court judge has power (jurisdiction) to try any case for the recovery of land with an annual value not greater than £100. But when the law talks of things that anyone may do, if they are properly qualified, the word 'capacity' is more commonly used than the word 'power'. When I married, no statute, no rule of common law, no private person empowered me to do so. I did not need any such power: I merely

had the requisite capacity. If you are insane, you have not the capacity to make a will, and if you are under the age of 16 you have not, in English law, the capacity to marry. Accordingly people are elected or appointed to the exercise of powers, but capacities are merely acquired whether naturally or by a change in one's social circumstances or by a change in the law. Powers are sometimes delegated, capacities never. Powers are surrendered or revoked, but capacities are lost or restricted by disqualifications. The people who wield powers may be impersonated, but with regard to a capacity all one can do is to pretend to have it when one has not. One may or may not have the capacity to exercise a power, but there is no such thing as the capacity to exercise a capacity. In a textbook on the law of marriage, the heading 'capacity' directs attention to the rules determining *who* is capable of marrying; in a textbook on the law of local government, the heading 'powers' would direct attention to the rules determining *what* a local authority may do.

I am not suggesting that Hart at any time confuses the concept of a power with that of a capacity, though he does not in fact articulate their differences. Indeed, when he remarks that 'behind the power to make wills or contracts are rules relating to capacity or minimum personal qualification (such as being adult or sane) which those exercising the power must possess' (28), it is quite clear that he distinguishes a power from a capacity. Where he creates perplexity is about the extension of the term 'power', not about its intension. Powers to make wills or contracts, or to marry, are, except in special cases, powers unknown to the law, which treats rather of capacities here. Presumably the reason for this is that wills, contracts and marriages are transactions that most people will want, and are suited, to perform anyway in a modern community, so that the law treats most people as being capable of performing them in any case and contents itself, in effect with laying down certain disqualifications. But where a power is conferred, as on Chief Constables, or where provision is made for the conferring of a power, as for the appointment of executors, the law assumes rather that most people do not want, or are not suited, to exercise the power unless certain special conditions are fulfilled. If a grant of power (from some person or authority) is needed to make some testamentary disposition, it is because the testator would not otherwise be entitled to dispose of the property in question. Hence to assert that there are powers of making wills or contracts is to spread an appearance of unity over the whole range of what Hart classifies as secondary rules, while little unity really exists, for the term 'power' is extended by Hart to cover almost everything regulated by law that is not a duty. In a work designed to further the understanding of law (vi) in a work that objects to the theory that all rules impose duties because such a theory achieves uniformity at the price of distortion (38), in a work that condemns the use of the term 'morality' as a conceptual waste-paper basket for all rules of social behaviour that are

not legal (222)—thus treatment of the term 'power' requires a justification which Hart never adequately provides.

More specifically, in English legal jargon what is called a 'private power' is an authority which one person gives to another and is the only form of power apart from public powers, which are authorities from the Crown, Parliament, etc for a public purpose (cf A. W. Motion, *Pocket Law Lexicon*, 1951, and G. R. Hughes, *The Student's Law Dictionary*, 1936, s.v Power). To call an individual's general competence to make wills, contracts, etc, 'a private power', as Hart does on several occasions (40, 77, 79, 96, 239), is therefore even less helpful for students of English Law than it may be for the students of systems in which no such expression as 'private power' functions as a technical term. There is even a discrepancy between Hart's use of the term 'public power' and the ordinary legal use of it in England. If public powers are only authorities from the Crown, Parliament, etc, than one can hardly speak in general of what the Crown or Parliament may themselves do as public powers. Admittedly usage may not be quite uniform here. Anson, *The Law and Custom of the Constitution* (5th edn 1922, p 340) does speak of the Crown's having at one time 'assumed to itself legislative powers', and some dictionaries of English law, e.g. W. J. Byrne's (1923) and Earl Jowitt's (1959) speak of private powers as those conferred on private persons, whether by other persons or by statute. But a power of ordinary citizens to marry, contract or make wills seems quite unknown to the Common Law.

Certainly Hart errs in good company here. Bentham spoke of people in general as being 'empowered' by the law to make wills (*Limits of Jurisprudence Defined*, ed C. W. Everett, 1945, p. 54), and he said that buying and selling was a mutual exercise of what he called 'the investative power' by two persons in favour of each other (*op. cit.* p 75). But Bentham at least had the excuse that he was fundamentally at variance with the existing legal system and was planning a new one, whereas Hart claims to be elucidating law as it is. Again, Salmond instanced the right to make a will or the right to marry one's deceased wife's sister as a private power (*Jurisprudence*, 11th edn 1957, pp 273 f), Hohfeld spoke of a citizen's power to create contractual obligations (*Fundamental Legal Conceptions*, 1923, p 51); Roscoe Pound has spoken of the power to acquire ownership of other people's property through ordinary legal transactions (*Jurisprudence*, 1959, IV p 99), and Kelsen, *General Theory of Law and State*, trans. A. Wedberg 1946, p 138) spoke of the law as empowering people to make legal transactions. But all this shows is that the term 'power' as Hart applies it is a term of jurisprudence, not that it is a term of law, and it is remarkable how successfully English legal terminology has resisted this juristic usage and retained its own, much narrower application of the term. Nor is it easy to see how Hart could defend his use of the term 'power' in this juristic fashion (let alone his failure to draw attention to the

differences between this use and ordinary legal usage), since it is a principle purpose of his theory to elucidate 'the specifically legal concepts with which the lawyer is professionally concerned' (95).

### III

What defences could Hart put up here? I can think of five, but there may be more, and better, ones

First, the boundaries of strictly professional legal usage are not absolutely clear, and Hart might help his case by pointing this out. No doubt the jargon of parliamentary draftsmen falls well within those boundaries and much of Kelsen's or Bentham's terminology falls outside. But, in between, the strictness of legal usage fades gradually as one passes from the language of textbook writers and judges through that of barristers and solicitors into that of jurists and philosophers. It would be surprising if there were not at least some professional lawyers who from time to time use the term 'power' as many jurists have done. Nevertheless Hart's use of that term gets more and more difficult to illustrate as one passes from the less strict fields of usage to the more strict ones. It is very difficult indeed to find instances of it in recent English statutes or text-books.

Secondly, Hart might claim that even if modern legal systems do not explicitly confer powers to marry or make contracts, yet, when looked at from the outside, this is what they appear to do. 'Even if the term "power" would be quite inappropriate here as a term of law', the defence would run, 'yet as a term of sociology it is exactly right. Weddings and business agreements would have little effect in a modern community unless they had the force of law behind them: it is the law that gives them what power they in actual fact have'. But this defence is not open to Hart, if his distinction between primary and secondary rules is to fill the role for which he casts it. For he tells us that 'rules conferring private powers must, if they are to be understood, be looked at from the point of view of those who exercise them. They appear then as an additional element introduced by the law into social life over and above that of coercive control. This is so because possession of these legal powers makes of the private citizen, who, if there were no such rules, would be a mere duty-bearer, a private legislator. He is made competent to determine the course of the law within the sphere of his contracts, trusts, wills, and other structures of rights and duties which he is enabled to build' (40, 94). Thus for Hart's theory it is essential that the term 'power' be a term of law here not of sociology—a term suited to the internal point of view, not the external. Indeed, Hart explicitly rejects the view that an analysis of these powers should be conducted 'in the terms of ordinary or "scientific" fact-stating or predictive discourse'. 'To do justice to their distinctive, internal aspect', he tells us 'we need to see the different ways in which the law-making-operations of the legislator, the

adjudication of a court, the exercise of private or official powers, and other "acts-in-the-law" are related to secondary rules' (96) Moreover, Hart is careful to point out that his account of primary rules as duty-imposing is in conformity with Anglo-American legal usage, since in this usage, he says, the terms 'duty' and 'obligation' are now roughly synonymous (238) One might therefore suppose his account of secondary rules as power-conferring to claim an equal accordance with legal usage

Thirdly, Hart might defend his use of the term 'power' with the argument that, though it does involve some distortion here, yet the distortion is illuminating and harmless, because it points to the fact that possession of these legal powers makes of the private citizen a private legislator (40) But to extend the concept of legislation in this way, *pari passu* with the concept of power, so that testators, contractors, trustors, and even brides and bridegrooms become private legislators, is radically to blur the concept of legislation A legislature may make new laws or repeal old ones Thereby it changes the content of a legal system A private citizen may, by his wills, contracts, trusts, marriages and other transactions bring himself and others within the range of certain laws but he does not change, or even amplify, their content Hart uses a number of blanket-phrases which have the effect of destroying this distinction Private legislators, like public ones, he says, 'determine the course of the law' (40), 'create structures of rights and duties' (27), 'introduce new rules of the primary type, extinguish or modify old ones, or in various ways determine their incidence or control their operations' (79) But to lose the everyday distinction between changing the law, on the one hand, and bringing individual persons or acts within its range, on the other, is a high price to pay for Hart's use of the term 'power' It is difficult to see what illumination is gained therefrom

Admittedly there was a time in the mid-nineteenth century when the influence of Bentham and Ricardo may have favoured the conception of a contract as a piece of private law (cf G O Cheshire and C H S Fifoot, *The Law of Contract*, 5th edn, 1960, p 21) The doctrine of *laissez-faire* required the utmost freedom of individuals to contract with one another as they wished, and if contracts, rather than Parliaments, were thus to regulate how men lived, it was not altogether absurd to think of contract as an alternative legislative procedure to Parliament (though if the analogy were pressed hard the doctrine of consideration in the English law of contract might seem to suggest that all contractors were corrupt legislators!) Bentham himself wrote that when the law gives a power of occupation over a piece of land 'it in a manner adopts the expression of my will, and turns it into a law' (*op cit* p 73) But the ideal as well as the reality of *laissez-faire* has long since been destroyed by two important developments in modern society Generations of statutory restrictions on freedom of contract in

matters of employment, trade, and industry have made it clear that freedom of contract is not a paramount consideration in the eyes of the state, and that contract is therefore to be regarded as an object of legislation rather than a creator of it. In a welfare-state age there is not much illumination to be gained from thinking of contracts as pieces of private legislation. In addition, the success of standardised forms of contract—with their attitude of take it or leave it—in many fields of social or economic activity has made it apparent that even within the room for manoeuvre left open by the state freedom is often completely one-sided. If there is any point at all in talking about private legislation here, the original act of drawing-up and promulgating a standardised form of contract should be regarded as being like a legislative activity, not the individual contracts that are subsequently made in conformity to such a standard. Indeed in *Bonsor v Musicians' Union*, where the relation between the parties was regulated by a document contractual in form but in substance a series of peremptory rules, Lord Justice Denning actually remarked of these rules that 'they are not so much a contract as we used to understand a contract, but they are much more a legislative code laid down by some members of the union to be imposed on all members of the union. They are more like bye-laws than a contract' (cf Cheshire and Fifoot, *op cit* p 24). To classify all contracts as pieces of private legislation, as Hart does, will thus make it specially difficult to reveal the true nature of a very large number of contracts in modern law, though even here we must be careful to say, as Lord Denning did, that the rules of a standardised form of contract are like bye-laws, not that they are such. Hart claims in support of his view that Kelsen has shown 'how many of the features which puzzle us in the institutions of contract or property are clarified by thinking of the operations of making a contract or transferring property as the exercise of limited legislative powers by individuals' (94, 245). But it seems to me that my criticisms of Hart here are equally valid against Kelsen. Moreover, when one looks at the passage of Kelsen to which Hart refers (*op cit* p 138), it is difficult to find any substantial use made of the analogy between contract and legislation except to support the thesis that, in general, individual legal duties under criminal law are stipulated directly while under civil law individual legal duties are stipulated only indirectly, through the medium of legal transactions. But this general principle does not in fact help us very much in distinguishing between civil and criminal law. Kelsen thought that the typical exception to his principle was the duty to repair damage caused illegally, and Hart himself alludes to duties of care under the law of tort (27). But in fact the principle has been well-nigh destroyed by modern welfare-state legislation. It is just in the course of legal transactions—in the sale of goods, the hiring and firing of employees, the formation of companies, the purchase of overseas investments, and so on—that so many important duties under criminal law now

arise If Kelsen's principle of distinction between civil and criminal law is the kind of 'clarification' we shall get by adopting the analogy between contract and legislation, we shall be better off if we do not adopt this analogy at all

Fourthly, perhaps, in order to save his dichotomy of primary and secondary rules, Hart might claim that the use of the particular word 'power' in his account of secondary rules was not important Any other term would do, he might say, that would cover the same range of rules On this defence, however, the supposed analogy between contracts, wills, marriages etc on the one side, and legislation, on the other, would need to be dropped, and it would not be at all clear what there was in common between rules determining capacities for marriage or contract and rules conferring legislative or judicial powers The attempt to find another term or expression embracing both, such as 'right', 'liberty' or 'privilege', would encounter similar difficulties, and in the end it is doubtful whether anything would be found that all Hart's secondary rules have in common except the more or less negative property that they are not in themselves duty-imposing rules though related to them in various ways For example, at one point (9) Hart distinguishes between mandatory rules and rules which indicate what people should do to give effect to the wishes they have But, though the latter description applies to many of the rules prescribing the procedures, formalities and conditions for the making of marriages, wills or contracts, it hardly applies to rules conferring legislative or judicial powers Elsewhere Hart points out that if a transaction does not conform to a relevant secondary rule the transaction is invalidated but the non-conformity to rule does not itself constitute a legal offence (28-31) There is a nullity, but not a breach of duty Apart from the 'complication' that in the interest of public order a judicial decision that has been given in excess of jurisdiction stands, at least in England, until quashed by a superior court, the position is just the same in this respect, Hart argues, for what he calls 'public powers' as for what he calls 'private powers' (30) But the difference Hart mentions seems a little more than just a complication The fact is that, even though a decision in excess of jurisdiction is liable to be quashed on appeal, it will nevertheless remain valid for ever if no appeal against it is made But wills and marriages are not like this at all If I do not comply with s 9 of the Wills Act 1837, I have not made a will at all no action by others is required in order to nullify it Legislation presents us with yet another situation If a measure proposed before a legislature fails to secure the requisite majority of votes, which Hart regards as a breach of a secondary rule (31), no action has been nullified or invalidated The measure was properly proposed, seconded, opposed and debated The vote was properly taken All that has happened is that the proposal has been unsuccessful Admittedly if a local authority disobeys certain rules about making bye-laws the result may in some cases be a nullity But the difference between this

nullity and the nullity of an unwitnessed will is as great as the difference between the corresponding valid transactions, of which one is an act of legislation and the other, *pace* Hart, is not. Of course Hart would be right to insist here, as he does in his book (15, 234), that for a general term, like 'law', 'crime', 'just', 'good', etc., to be correctly used it is not essential that the range of instances to which it is applied should all share common qualities. But he must at least suggest some central model, or some range of useful analogies, which would justify grouping together such *prima facie* heterogeneous rules as those he includes under his rubric of 'secondary rules', and it is already clear that the alleged analogy between contract and legislation is somewhat remote.

Fifthly, therefore, Hart might be content with a rather more negative account of what secondary rules have in common, viz their not being primary ones though related to them in various ways, and he would then rely on a disjunctive account of these relations in order to give body to his distinction. Secondary rules, he might say, either determine how primary rules, or other secondary ones may be created, changed, or abolished, or how they may be administered, or how people may come within their range. They are all 'parasitic' on primary rules (79), though parasitic in different ways. But the trouble then is that it is difficult to see why one should not treat what Hart calls secondary rules merely as conditions on primary ones. If they can thus be regarded as various different kinds of parasites of primaries, they might just as well be treated as parts of primaries. After all, Hart himself sometimes speaks of secondaries as 'defining the ways in which valid contracts or wills or marriages are made' (27), or asserts that their concern is 'to define the conditions and limits under which the court's decisions shall be valid'. The ease with which it is possible to describe the function of secondary rules as a defining one suggests that there may be better excuse than Hart allows for thinking of the law as a system of primary rules only—as a system of rules that impose duties and determine sanctions for their breach.

Hart does in fact examine three *prima facie* plausible ways of reducing what he calls secondary rules to what he calls primary ones. He grants that there are points of resemblance between these two sorts of legal rules. In both cases actions may be criticised or assessed by reference to the rules as legally the 'right' or 'wrong' thing to do. 'Both the power-conferring rules concerning the making of a will,' he says, 'and the rule of criminal law prohibiting assault under penalty constitute standards by which particular actions may be thus critically appraised' (32). But he denies the possibility of reducing secondaries to primaries.

According to one theory the nullity which ensues when some essential conditions for the exercise of a power is not fulfilled is like the punishment attached to criminal law. It is a threatened evil or sanction exacted by law for breach of the rule. We are to think of



the rule providing that a will without two witnesses will be inoperative, as moving testators to compliance with s 9 of the Wills Act, just as we are moved to obedience to the criminal law by the thought of imprisonment. Against this theory Hart rightly objects not only that in many cases nullity may not be an evil to the person who has failed to satisfy some condition required for legal validity (33). More seriously, he points out, in the case of a rule of criminal law we can identify and distinguish two things—a certain type of conduct which the rule prohibits, and a sanction intended to discourage it. But we can hardly consider in this light such desirable social activities as men making each other promises which do not satisfy legal requirements as to form. Again, in the case of criminal law we can, in a sense, subtract the sanction and still leave an intelligible standard of behaviour which it was designed to maintain. But we cannot logically make such a distinction between the rule requiring compliance with certain conditions, e.g. attestation for a valid will, and the so-called sanctions of 'nullity'. In this case, if failure to comply with this essential condition did not entail nullity, the rule itself could not be intelligibly said to exist even as a non-legal rule (34).

Hart also considers Kelsen's theory that what is ordinarily thought of as the content of law, designed to guide the conduct of ordinary citizens, is merely the antecedent or 'if-clause' in a rule which is directed not to them but to officials, and orders the latter to apply certain sanctions if certain conditions are satisfied. All genuine laws, on this view, are conditional orders to officials to apply sanctions. The provisions of the Wills Act requiring two witnesses would appear as a common part of many different directions to courts to apply sanctions to an executor who, in breach of the provisions of the will, refuses to pay the legacies 'if and only if there is a will duly witnessed containing these provisions and if then sanctions must be applied to him'. The provisions of the U.S. constitution as to the law-making power of Congress would merely specify the general conditions under which courts are to apply sanctions. But, as Hart rightly objects, we shall conceal the characteristic way in which the rules of criminal law function if we concentrate on, or make primary, the rules requiring the courts to impose sanctions in the event of disobedience. These latter rules are only ancillary, in that they make provision for the breakdown or failure of the primary purpose of the system. After all, the characteristic technique of the criminal law is to designate by rules certain types of behaviour as standards for the guidance either of the members of society as a whole or of special classes within it—they are expected without the aid or intervention of officials to understand the rules and to see that the rules apply to themselves and to conform to them.

But Hart also examines a third way of treating all legal rules as rules imposing duties, which he attributes to Bentham (239). According to this theory the criminal law and all other laws imposing

duties would be regarded in the normal way and not as Kelsen regards them. But 'rules which confer legal powers on private individuals' are, for this as for Kelsen's theory, 'mere fragments of the real complete laws—the orders backed by threats. These last are to be discovered by asking what person does the law order to do things, subject to a penalty if they do not comply?' When this is known, the provisions of such rules as those of the Wills Act, 1837, in relation to witnesses, and other rules conferring powers on individuals and defining the conditions for valid exercise of these powers, may be recast as specifying some of the conditions under which ultimately such a legal duty arises. Rules relating to the formation of contract will similarly appear as mere fragments of rules ordering persons, if certain things are the case or have been said or done (if the party is of full age, has covenanted under seal or been promised consideration), to do the things which by the contract are to be done. Similarly the rules conferring legislative powers are represented by the antecedents or if-clauses of rules ordering ordinary citizens, under threat of sanctions, to do things and not merely, as in Kelsen's theory, as the if-clauses of directions to officials to apply sanctions (37).

Against this Benthamite theory Hart's objections do not appear very strong. In regard to rules conferring 'private powers' he claims that such rules are thought of, spoken of and used in social life differently from rules which impose duties, and they are valued for different reasons. What other tests for difference in character, he asks, could there be? (41). But the trouble here is that, if I am right, there are no rules conferring private powers of the kind that Hart assumes: there are only rules regulating capacity. But there are also, in regard to contract, say, rules of offer and acceptance, rules of consideration, rules of content, rules of discharge, and so on. All these rules are thought of, spoken of and used in social life differently from rules of capacity and they are valued for different reasons. So if we are to distinguish rules of capacity from rules imposing duties for the reasons that Hart gives in respect of his supposed power-conferring rules, we should also distinguish all these other kinds of rules both from one another and from rules of capacity and from rules imposing duties. There would be no neat dichotomy of primary and secondary rules, but as many different kinds of rules as the complexity of our legal system allows. Hart seems to treat all these other rules as conditions on the exercise of a supposed power to contract. For him they seem to be antecedents of a conditional rule which has the form 'A contract may be made if and only if. . .' But he gives no reason why we should regard this pattern of conditionalisation as somehow less objectionable than Bentham's. Similarly with regard to wills, marriages, trusts, etc.—if rules of form, say, may be treated as conditions on the exercise of a power, then rules of power (or, more properly, rules of capacity) may be regarded as conditions on the existence of circumstances in which duties arise. One can either go all the way in one's logical reconstruction of the

law, and treat all law-sentences as either duty-imposing rules or conditions. ('parasites') on such rules. or alternatively one can draw as many distinctions and construct as many different schemes of classification as the richness of the law allows and the subtlety of our jurisprudence renders profitable. Indeed even in criminal law few duties or obligations arise that English lawyers would explicitly recognise as such. murder is a crime but the law imposes no duty not to murder.

Hart also argues against Bentham's reduction of the rules that confer and define legislative and judicial powers in the public sphere to statements of the conditions under which duties arise. He rightly remarks that those who exercise these powers to make authoritative enactments and orders use these rules in a form of purposive activity utterly different from performance of duty or submission to coercive control. But he is not quite right to infer from this that to represent such rules as mere aspects or fragments of the rules of duty is, even more than in the private sphere, to obscure the distinctive characteristics of law and of the activities possible within its framework (41). For those who wish, like Bentham, to represent such rules as fragments of the rules of duty are certainly not suggesting that the exercise of legislative power, say, should be regarded as a species of duty-performance or submission to coercive control. Certainly Bentham himself does not say, or suggest, in the work to which Hart refers (*Limits of Jurisprudence Defined*), that legislators are 'obeying rules when, in enacting laws, they conform to the rules conferring their legislative powers' (109). Just the opposite. Bentham and those like him are saying that rules conferring legislative powers are 'mere aspects or fragments of the rules of duty', in Hart's own terms, not complete rules of duty. On the Benthamite view rules conferring legislative powers turn into the antecedents of conditional rules with a form like, say, 'if the Queen in Parliament has ordained that . . . , then it is obligatory that . . . ', not into categorical rules of the form 'It is obligatory on the Queen in Parliament to . . . '. There need be no distortion or obfuscation here. Rather the true nature of duty-imposing rules is brought into relief: they are only valid if the Queen in Parliament . . . or if . . . etc. After all, the purpose of Hart's legal philosophy is presumably to turn a searchlight on the nature of the law rather than on the nature of legislative activity.

#### IV

Hart tries to sustain his concept of secondary rules by stressing how important was the introduction into society of rules enabling legislators to change and add to the rules of duty, and judges to determine when the rules of duty have been broken. This step, he says, may fairly be considered as the step from the prelegal into the legal world (41). Hart remarks that it is possible to imagine a society without a legislature, courts or officials of any kind. Indeed,

he says, there are many studies of primitive communities which not only claim that this possibility is realised but depict in detail the life of a society where the only means of social control is that general attitude of the group towards its standard modes of behaviour in terms of which Hart has characterised rules of obligation (89). Or at least there are studies of primitive communities which describe them as approximating to this state (244). Hart refers to such a social structure as one of primary rules of obligation, and he holds that if a society is to live by such primary rules alone there are certain conditions which, granted a few of the most obvious truisms about human nature and the world we live in, must clearly be satisfied. The rules must contain some restrictions on the free use of violence, theft and deception; those who reject the rules except where fear of social pressure induces them to conform must be in a minority, and the community must be small, closely knit by ties of kinship, common sentiment and belief, and placed in a stable environment (89). Such a system of social control by primary rules alone has three main defects. If doubts arise as to what the rules are or as to the precise scope of some given rule, there will be no procedure for settling this doubt, either by reference to an authoritative text or to an official whose declarations on the point are authoritative. This defect in the simple social structure of primary rules Hart calls its *uncertainty*. A second defect he calls the *static* character of such rules (90). There is no means in such a society of deliberately adapting the rules to changing circumstances, either by eliminating old rules or introducing new ones. The third defect he discerns in this simple form of social life is the *inefficiency* of the diffuse social pressure by which the rules are maintained. Punishments for violations of the rules are left to the unorganised efforts of private individuals; and, even more seriously, disputes as to whether an admitted rule has or has not been violated will always occur and are liable to continue interminably, if there is no agency specially empowered to ascertain finally, and authoritatively, the fact of violation (91).

The remedy for each of these three main defects in the simplest form of social structure consists, according to Hart, in supplementing the primary rules of obligation with secondary rules. The three remedies together are enough to convert the regime of primary rules into what is indisputedly a legal system, and thus it can be shown how 'law may most illuminatingly be characterised as a union of primary rules of obligation with such secondary rules' (91). The simplest form of remedy for the *uncertainty* of the regime of primary rules is the introduction of what Hart calls a 'rule of recognition'. This will specify some feature or features possession of which by a suggested rule is taken as a conclusive indication that it is a rule of the group and is to be supported by the social pressure which the group exerts. It may say no more than that an authoritative list or text of the rules is to be found in some written document or carved on some

public monument Or in a more developed legal system it may identify as authoritative any rule enacted by a specified body, any rule customarily observed in certain fields, and any rule acknowledged in judicial decisions, and provision may be made for possible conflict between these criteria by their arrangement in an order of superiority, as by the common subordination of custom or precedent to statute (92) The remedy for the *static* quality of the regime of primary rules consists in the introduction of what Hart calls 'rules of change' The simplest form of such a rule is that which empowers an individual or body of persons to introduce new primary rules for the conduct of the life of the group, or of some class within it, or to eliminate old rules, and more complex rules may limit these powers in various ways or define the procedure to be followed in legislation (93) The third supplement to the simple regime of primary rules, intended to remedy the *inefficiency* of its diffused social pressure, consists of secondary rules empowering individuals to make authoritative determinations of the question whether, on a particular occasion, a primary rule has been broken Besides identifying the individuals who are to adjudicate, such rules will also define the procedure to be followed, and they will normally be supplemented by yet further rules conferring upon judges, where they have ascertained the fact of violation, the exclusive power to direct the application of penalties by other officials (94, 95)

But it would in several respects be not quite right to characterise the shift from a pre-legal to a legal system in terms of the addition of secondary rules to original primaries Secondariness is neither a necessary nor a sufficient qualification for being a rule that needs to be added here

It is not a necessary qualification, since even some of the rules that Hart mentions are not secondary rules according to his definition of 'secondary' The rules he calls rules of recognition are not rules that confer powers, whether public or private they set up criteria They determine the sources of law they do not give power to someone to make it, like the rules that Hart calls 'rules of change' Sometimes, *e.g.* in the Roman-Dutch law of South Africa, a document or textbook which had little or no authority in a given community at the time it was written, or could have had no authority at all because the community scarcely existed, is later recognised by that community as an authoritative source of law. In such a case it borders on absurdity to suppose that the relevant rule of recognition constitutes a retrospective, and probably posthumous, grant of legislative powers to the author of the document or textbook Perhaps Hart does not wish us to suppose this But how then can we treat his rules of recognition as being secondary rules? Again, the law of evidence is a very important branch of any modern legal system, and volumes are written on it as large as any on the law of contract, family or real property Hart does not mention it specifically, and presumably it is to be taken as included

in what he calls the rules of adjudication, which, 'besides identifying the individuals who are to adjudicate, . . . will also define the procedure to be followed' by them (94). But this implies that the law of evidence should be treated as a mere part or adjunct of the rules conferring adjudicatory powers, and a student of English law at least would find out very little about the rules of evidence if he merely searched the statutes and precedents conferring jurisdiction. Look through the table of statutes and the table of cases in a standard work like *Phipson on the Law of Evidence* (9th edn 1952), and you will find that only a minute proportion of the references there are in any way concerned with matters of jurisdiction. Indeed it is not only a historical fact that the law of evidence has often developed independently of rules of jurisdiction. It is also arguable that something important would be lost if the law of evidence could not so develop. Hart might perhaps object here that rules of evidence do not lie at what he calls 'the centre of a legal system' (96). But rules of evidence are not of much less importance than rules of jurisdiction in remedying what Hart calls the *inefficiency* of a pre-legal system. They are essential if the facts about violations of primary rules are to be determined in a way that will inspire general respect. No doubt the rules of evidence, too, could become antecedents in a systematic reconstruction of the law on Benthamite lines as a set of conditional rules of duty. But Hart's method of making rules of evidence the antecedents of conditional rules of jurisdiction is no better suited than Bentham's to the project of showing how 'the specifically legal concepts with which the lawyer is professionally concerned . . . [are] best elucidated' (95).

These two examples—rules of recognition and rules of evidence—make it clear that secondariness is not a necessary qualification for being a rule that normally needs to be added in order to transform a pre-legal system into a legal one. Nor is it a sufficient one. Certain rules of a kind that Hart elsewhere classifies as secondary (27) appear in almost every pre-legal system. Hart himself refers to studies like B. Malinowski's *Crime and Custom in Savage Society* (1926) and A. S. Diamond's *Primitive Law* (1935) which depict in detail the life of a society where there are no courts, officials or legislature (89, 244). But are there very many such studies which depict a society where there are no rules of marriage? Indeed it is very often the case that the more primitive and pre-legal a community appears, the more elaborate are its rules determining capacity for marriage. No small part of cultural anthropology is concerned with the structure of these rules and their relationship to the life of a community as a whole. Malinowski (*op cit* p 35) remarks of the culture he studied that 'marriage establishes not merely a bond between husband and wife, but it also imposes a standing relation of mutuality between the man and the wife's family, especially the wife's brother'. Even Diamond remarks (*op cit* p 192) that there are already rules of marriage in the savage societies which do not possess courts.

The moral institution of a promise is thus far from being the only form of 'power-conferring' rule that is to be found in a pre-legal society, as Hart at one point implies (94). If there are obligations at all in such a society, then some of them may be created by marriages, transfers of property, and so on. It is no more an amenity of law that it enforces obligations so created, than that it enforces other obligations: if unorganised social pressure can enforce an obligation not to murder or steal, it can also enforce the obligations that arise out of marriage. Nor is it open to a defender of Hart's theory to argue that even if all actual primitive societies have rules of marriage, yet one can at least imagine a primitive society that has none. Hart himself remarks that 'in an extreme case the rules may be static in a more drastic sense. . . . Not only would there be no way of deliberately changing the general rules, but the obligations which arise under the rules in particular cases could not be varied or modified by the deliberate choice of any individual' (90). For, if people are to be released from certain obligations or if they are to transfer to others the benefits which would accrue from performance of their own obligations, 'there must be rules of a sort different from the primary rules': there must be rules creating changes in the initial positions of individuals under the primary rules of obligation (91). However Hart also recognises that such a state of affairs is 'never perhaps fully realised in any actual community', and it is doubtful if we can gain much understanding of the actual transition from pre-legal to legal by considering a state of affairs that is not only never in fact fully realised but is also fundamentally alien to much of the primitive human life that is revealed in anthropological studies. No doubt rudimentary or embryonic organs of legislation or adjudication may be found in very many primitive societies that we should still wish to call pre-legal, but if the institution of marriage exists at all in such societies it is relatively unlikely to be as rudimentary or embryonic.

It is true that I have myself already argued that the law of marriage, contract, etc., does *not* consist of rules conferring powers on individuals to marry, make contracts, etc., and is therefore misclassified by Hart as a set of secondary rules in his sense. But a defender of Hart's theory would seem to be caught in a dilemma here. If he gives up treating the law of marriage, contract, etc., as a set of secondary rules, then the extension of the term 'secondary rule' is more or less confined to rules of legislation, jurisdiction and administration. Since this looks suspiciously like the three branches of law traditionally treated as constitutional law, the term 'constitutional' would then perhaps be an apter designation of these rules. But then Hart's claim that his distinction between primary and secondary rules is the key to the science of jurisprudence and has great explanatory power must be reduced here to the somewhat less exciting proposition that the addition of a constitution, setting up legislature, judiciary and executive, is the most important element

in the transition from a legal to a pre-legal system. On the other hand, if a defender of Hart's theory insists on treating the rules of marriage, contract, etc., as power-conferring rules, then he must not claim that the primary-secondary distinction casts any great light on this transition. There are secondary rules in any ordinary pre-legal system as well. What is important about the secondary rules required for transition to a legal system is not the fact that they are secondary (indeed, some of the rules so required, as we have seen, are not secondary ones at all) but the fact that they set up a constitution. To suppose that the secondariness of the rules is important here, rather than their constitutional character, is like supposing that what is important in creating the traffic problem of the mid-twentieth century is the invention of the wheel—if I may adapt a simile of Hart's (41)—rather than the mass-production of power-driven vehicles. The only way of escape through the horns of this dilemma seems to be to assume, as Hart sometimes does (89, 94), not only that the law of wills, contracts, marriages, property, etc. consists of power-conferring rules, but also that wills, contracts, marriages, property, etc. are institutions which exist only in a fully developed legal system, just like legislatures and law-courts. But, as I have already suggested, such an assumption is not in accordance with the findings of modern anthropology.

## V

In short, it is not at all clear how anyone could successfully defend Hart's claim to have found the key to the science of jurisprudence in his distinction between primary and secondary rules. But it is very much to be hoped that, if Hart himself still believes this, he will amplify his theory at some convenient opportunity and show how it is immune to criticisms of the kind that at present seem relevant. So far as can at present be seen the main merit of the theory lies in the fact that it forces anyone who wants to criticize it to rethink his own attitudes over the whole field. That is no inconsiderable merit for a philosophical theory to have, but it is not the merit that Hart himself claims for his theory. Indeed there may be room for doubt about how far Hart's philosophical ideal of elucidating legal concepts as they are is compatible with the discovery of any single principle as the key to the science of jurisprudence.

Because I have been putting forward these criticisms of Hart's central thesis, I have not mentioned in detail the very large number of more defensible points that Hart makes in other connections. Perhaps the final chapter, on international law, is also a little disappointing, since while Hart refutes various arguments for denying that international law is a legal system, or for denying value to the question whether it is or is not a legal system, he does not discuss in detail any analogies of content between municipal and



international law, despite his claim that in such analogy no other social rules are so close to municipal law as those of international law. But in his analysis of the difference between a rule and a command, in his replacement of the concept of legal sovereignty by the concept of an ultimate rule of recognition, in his insistence on the importance—*vis-à-vis* juristic positivism—of the difference between the internal and the external aspect of rules, in his discussion of the relation between law and morality, in his exploration of the narrow path between formalism and scepticism as rival attitudes to the relation of judicial decisions to rules, and in his very fair examination of how solid a basis there is for theories of natural law—his book has made a considerable number of profoundly important contributions to the philosophy of law

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*Induction and Hypothesis A Study of the Logic of Confirmation*  
By S. F. BARKER. Cornell University Press (London. Oxford University Press) Pp 203 + xvi 22s

THE argument of this study of non-formal reasoning is conducted largely in abstract philosophical and logical terms. It examines doctrines and theories about arguments rather than actual instances of reasoning. In most cases the doctrines and theories are sketched rather than elaborated in detail and counter-arguments and objections are treated in the same way. This method of argument allows Professor Barker to compress a great deal of material into a short book, but at the cost of some loss of conviction at key points in the discussion. In particular the method sometimes leads to a failure to analyse questions in sufficient detail so that important distinctions disappear. In this Barker is not alone, since many writers on induction have created difficulties by seeing only one hard question where a more detailed analysis can extract smaller questions which are not hard to answer. The lack of detailed examination of actual instances of scientific reasoning also contributes to the lack of conviction in the argument, since whatever is said about the logic of generalizing must be tested against instances of the most successful generalizing we know, the laws of nature.

Four phases can be distinguished in this discussion.

*Phase I* Attempts by linguistic analysts to dissolve the traditional 'problem of induction' are described and criticized, with the conclusion that there is a problem about the legitimacy of generalizing, and this residual problem is very closely similar to the traditional 'problem of induction', that is the problem is to find a *formal* scheme for valid generalizing.

*Phase II*: The classical solutions to the traditional problem are examined and dismissed in the standard manner. The introduction of over-riding premises to construct the inductive syllogism 'Over-

riding premise plus Particular facts implies Generalization' is shown to be question-begging on the standard grounds that whatever the over-riding premise—uniformity of nature, principle of limited variety or fair sampling assumption—it must be argued for by generalization of some particular information, and any such argument must itself require some other over-riding premise, which must be established by like methods, and so on.

*Phase III* Various positivist proposals for the restriction of the scope of generalization are examined. Both reductionism and formalism are dismissed on the grounds that neither allows as legitimate certain ontological claims which are ordinarily held to be legitimate, in particular claims that certain things are real and are the causes of phenomena become problematic on a reductionist or formalist programme. The ontological restrictiveness of the stricter forms of positivism is an unwelcome consequence since it forces us to dismiss as invalid many arguments which commonsense and the natural sciences sanction.

*Phase IV* Finally after rejecting various versions of the hypothetico-deductive method of sanctioning a generalization, a method based upon Kemeny's theory of 'logical measure' is proposed. Instead of looking for argument-forms which will enable us to infer a unique general statement from a set of particular statements, the traditional way of tackling the problem of finding logical support for generalizing, we are to look for methods for selecting from among a potential infinity of hypotheses the most satisfactory hypothesis for any given set of particular facts. The particulars influence the choice of hypotheses through their role in determining which hypothesis from the set of possible hypotheses is the most simple.

Phases II and III of the argument do not contain much that is new or original. The interest of the book lies in Phases I and IV, the critique of the dissolution of the 'problem of Induction', and the attempt to construct a method for the selection of hypotheses that can be given a formal treatment.

The traditional view of inductive reasoning can be put something like this: suppose we have a set of objects,  $a, b, c$  which, for all we know, may be indefinitely large, but for which we do have a criterion,  $\phi$ , for determining whether any given object  $x$  belongs or does not belong to the set. For practical reasons we can only examine a finite sub-set of these objects, say  $a, b, \dots, n$ . On examination we find that all the objects  $a, b, \dots, n$ , have some property  $\theta$ , not identical with  $\phi$ . We reason inductively when we conclude

*Because  $a, b, \dots, n$  have  $\theta$*

$n + 1, \dots$  have  $\theta$  if they have  $\phi$ .

The question which inductive logic sets out to answer is 'How far do we strengthen the proposition "*All which have  $\phi$  have  $\theta$* " if we

examine some further finite subset ' $n + l, \dots n + k$ ' and find that all  $n + l, \dots n + k$  have  $\theta$  if they have  $\phi$ , i.e. if they belong to the same set as  $a, b, \dots n$ . Some suggested answers are

(1) If we assume the uniformity of nature we can conclude with deductive rigour that 'All which have  $\phi$  have  $\theta$ ' whatever number of objects we have examined

(2) If we assume the principle of limited variety, the property  $\theta$  is one of a finite set of possible properties which things of the kind  $\phi$  can have, so that the more instances in which we find  $\phi$  and  $\theta$  together, the less likely are we to find an object having  $\phi$  with one of the other possible properties,  $\psi$ , and since  $\theta, \psi, \dots$  is a finite set the calculation of the probability of any  $\phi$  having  $\theta$  is possible, and is not affected by the potential infinity of the objects exhibiting the properties in question.

(3) If we assume the principle of fair sampling then by choosing an object having  $\phi$  at random, and finding that it also has  $\theta$ , we strengthen the probability that every object which has  $\phi$  has  $\theta$

In each of these 'solutions' there is an assumption of a universal but empirical character, e.g. *in fact* regularities of coexistence of properties are universally exhibited, that there are no more than a finite set of possible properties of material objects, that it is possible to make a random selection and know that this selection has no special or unique accidental feature. Each assumption being empirical and universal is not provable with logical rigour, hence the method of inference governed by each assumption is not logically rigorous

To this the dissolutionist has a two-fold answer

(a) To the general objection that inference from particular empirical fact to generalization is not logically rigorous and that no good grounds for such inferences can be found, he replies that what we mean by 'good grounds' for such inferences can only be particular empirical facts, used judiciously in support of general empirical hypotheses—there can be no other way of carrying out an empirical investigation but empirically.

(b) To the particular objection that this or that specific inductive argument is not logically rigorous he replies that admittedly it does not satisfy the demands of deductive logic for rigour but it is not a deductive argument. There are many forms of inductive inferences which are, in fact, acceptable to the people using them, and are pragmatically successful, and which define standards of validity for themselves. Once we free ourselves from the deductive model we can consider each form of inductive argument by itself and investigate its validity, knowing full well that we shall never find that such an argument exhibits deductive validity

Barker raises the following objection to this method of dissolution. The dissolution confuses the process of inventing arguments with the process of evaluating them. Particular inductive inferences can only be studied as unique forms when we consider how they were originally made. When we come to consider whether they are

valid or not we *must* employ general rules of validity of some sort. He gives three supporting reasons for this objection.

(i) If we can intuitively distinguish between valid and invalid arguments and agree about our decisions then, in principle, a machine must be able to do the same job. Barker remarks cryptically 'any such task that can be done can be done by a machine' (page 22). Machines must be constructed according to general rules, hence the intuitive distinguishing, whether we know it or not, depends upon the application of general rules.

(ii) If we cannot intuitively distinguish and agree about decisions then we cannot seriously claim *validity* for any non-demonstrative inference.

(iii) Inspection of specific arguments does not always yield a clear cut decision upon their validity; and in such a case to make a decision the specific argument must be considered as an instance of a kind of argument, compared with others which seem to be similar in form, and so on, and this yields general rules.

If the dissolutionist's case was really as Barker expounds it these strictures would be sound. I think one must accept the first leg of the dissolutionist's case that, in effect, no *one* formal scheme could be found which would transform non-demonstrative into demonstrative arguments. It does not follow from this that each non-demonstrative argument is unique, and the decision on its validity intuitive. Barker misunderstands the dissolutionist's case if he supposes that they do wish to draw this conclusion. What does follow is that there may be several classes of non-demonstrative and acceptable inferences each capable of justification, but none capable of formalization in some quasi-deductive way. It follows from the first leg of the dissolutionist's case, not that every acceptable argument requires us to maintain a unique sense of validity for that argument, but that for each class of non-demonstrative argument there is an acceptable and unacceptable form, the acceptable form defining a sense of 'validity' for that form, and further that no one of these senses of 'validity' is identical with 'deductive validity'.

A further point of the greatest importance can be extracted from the dissolutionist's case. It is a point overlooked by Barker and in the end proves fatal to his whole enterprise. He says (p. 25) that in the study of induction we must advance 'towards an understanding of what logical principle ought to be regarded as fundamental to non-demonstrative inference'. But this already presupposes that there is a principle waiting to be discovered. It assumes that the logical form of non-demonstrative inferences is

Premises	}	Principle of Inference
↓		
Conclusion		

that is, the general form of deductions. It may well be that whether a particular move Premise—Conclusion is regarded as valid,

depends. not upon the *logical form* of the premises and conclusion, that is requires accordance with a logical principle for us to agree to its validity, but depends rather upon the *content* of the particular premises and conclusion. An important case when this is undoubtedly true is that when a non-demonstrative inference gains acceptance through its exploitation of sometimes extremely complex structures of implicit and explicit analogies : a very common feature of the sciences. If we try to see why the inference is acceptable by schematizing the propositions into abstract 'some' and 'all' forms. the reasons for the acceptability of the inference must escape us, since the acceptability depends upon what 'some' and what 'all' we are talking about. To put this in another way : a great many acceptable inferences in the sciences are intensional rather than extensional in the relations between premises and conclusions. Barker is right in supposing that the dissolutionists do not close the problem of induction. but if it is true that intensional relations between the terms of the propositions expressing our empirical knowledge determine the validity of non-demonstrative inferences, then he is wholly wrong in supposing that the residual problem of why some arguments are acceptable and others are not. is (a) one problem. and (b) that all solutions can be abstract and formal.

In Phase IV of the argument Barker proposes his own abstract and formal solution. If this can be faulted in confrontation with some actual examples of acceptable inferences then the way is once more open for the development of some form of intensional logic to deal with the validity decisions for arguments not susceptible to Barker's schematic rules

The argument of Phase IV is designed to lead us to a sense of 'confirmation' in which a new empirical fact can be said to add to or decrease the credibility of an hypothesis. Barker is not aiming at the sense of 'confirmation' meaning 'demonstrate as true'. In outline his method of reaching this sense of confirmation is as follows :

(i) For any set of empirical facts there are, in principle, infinitely many hypotheses which will account for them ; in the sense that given some member of the set of empirical facts, the other members are deducible from that member plus the chosen hypothesis or hypotheses

(ii) A procedure can be devised for ranking the hypotheses in order of simplicity. For any given universe the members of which can exhibit any of  $k$  predicates 'there is always an answer to the question, in how many ways would such-and-such statements of the language come out true if the universe contains just  $n$  members' (p. 178) An hypothesis is simpler than another if it comes out true for fewer possible set-ups or models of the universe.

(iii) This procedure allows us to give a formal sense to the simplification of a system achievable by adding an hypothesis. for by adding an hypothesis we can reduce the number of models for which the system of hypotheses plus the empirical evidence comes out true.

(iv) We can now give a sense to 'the superior confirmation of an hypothesis', for the more favourable instances there are the more successful is the hypothesis in reducing the number of models, since by adding these favourable instances to the system we increase the disparity in models between an hypothesis and any competitor. Barker shows that this abstract formal method of confirmation works, abstractly and formally, for hypotheses of the form 'All  $F$  are  $G$ ' and the form ' $(x)(y) \dots Rxy$ ', that is for class inclusion and relational hypotheses

But however ingenious this criterion may be it does not escape any of the old objections, for it can still be argued that it provides no reason why we should not turn up a new piece of evidence tomorrow which flatly contradicts the hypothesis and so adds to the number of models which the theory may have. Certainly at every stage in an enquiry we can say, by Barker's criterion, which hypothesis has the fewer models, and hence which is the better confirmed by the evidence *up to date*, but this gives us no guarantee whatever that further evidence will continue to be favourable. If we could find any such guarantee then the elaborate structure of Barker's criterion would be unnecessary since we could then proceed by the inductive method of generalizing our evidence to date. Logicians, including Barker, have queried the inductive method, just because the kind of guarantees which would make simple generalizing logically impeccable cannot be found by any other method than simple generalizing. But since Barker's criterion also requires such guarantees, it cannot be a genuinely non-inductive method of confirmation, even in Barker's weaker sense of 'confirmation'. Not only may contradictory evidence turn up, but we may be obliged to add new predicates to the system of scientific language, so that not only does Barker's criterion demand some form of the Principle of the Continuity of Regularities with time, but also some form of the Principle of Limited Variety, if it is to be applied at all. If Barker's criterion works for the sciences, then it works under the implicit guarantee of some such principles, but if it works, then so would simple generalizing, for given the principles, simple generalizing is legitimate too.

What we can discover about the world are .

- (1) Regularities of coexistence of properties
- (2) Invariances of properties or relations.

The logical problem of the sciences has always been what justifies anyone in concluding from a finite set of instances of regularity or invariance to an unrestricted generalization of the regularity or the invariance? Scientists do draw and have always drawn general conclusion from particular facts, and they have always regarded further instances of the particular facts as adding to the credibility of the generalization. What Barker has done is to provide us with a criterion for assessing the credibility of a generalization against the background of some given evidence, indeed I think it could be

shown that his criterion is a generalization of Mill's Canons from Aristotelian to quantificational logic. But the problem of induction is not by-passed by this device for nothing whatever follows about the degree of belief we are to attach to a *prediction* made from any generalization or hypothesis.

To make clear where I think the real problem lies it is necessary to distinguish between the *conditions* for the success of any intellectual process and the *postulates* of a logic in which the intellectual process can be formally modelled. In so far as we are considering a logical system purely formally we do not expect to be able to deduce the postulates of that system, using the system itself. To attempt to find some chain of reasoning leading from empirical evidence to the Regularity *postulate*, or the Limited Variety *postulate*, is to ask for something that logic cannot accomplish. But to ask for reasons for accepting those principles as *conditions* of generalizing is to ask a quite different sort of question to which an answer can be provided, though that answer will not be logic. Furthermore I think that the dissolutionists have shown that we cannot expect to find any non-trivial reasons for adopting these conditions of generalizing, if we ask the general question 'Why accept generalizations of empirical facts as a guide to the future behaviour of nature?' But we can answer questions of a more specific kind, 'Why expect an object having the property  $\phi$  always to exhibit the property  $\theta$  under some set of conditions  $\sigma$ ?' if we have on several occasions observed objects which have the property  $\phi$  exhibiting the property  $\theta$  under the conditions  $\sigma$ .

The standard form of answer in the natural sciences runs as follows

The property  $\theta$  is the result of the working of the internal mechanism of the object having the property  $\phi$ , stimulated by the conditions  $\sigma$ .

#### *Example A*

'Why do we expect the sufficient heating of a body to be always accompanied by the emission of light?'

*Answer:*

(i) Heat is a form of energy which is absorbed by the atoms of the substance (Stimulus)

(ii) The absorption of energy by an atom results in a 'jump' of the electrons of that atom into higher energy states. These states are unstable and electrons 'drop' back to their original energy states, emitting the energy difference as electro-magnetic radiation (Mechanism)

(iii) Some of this electromagnetic radiation is of visible wavelength (Response)

It is the mechanism which is constructed to behave in certain more or less definite ways that allows us to assert the regularity between

properties of the substance, that is that heating is accompanied by the emission of light

This mechanism guarantees the regularity. But what grounds have we for supposing that the mechanism will continue to behave regularly? To supply grounds we need to describe the mechanism of the atomic mechanism, and doubts about its regularity can be satisfied by describing the mechanism of the mechanism of the mechanism of the atom and so on. This is the kind of open ended guarantee that is actually to be found in the sciences. It is not that the limited number of examples of heating accompanied by light lead by means of some abstract logical principle that is very hard to discover to the generalization that sufficient heating is accompanied by the emission of light, but that by considering what heat is, and what light is, and what mechanism relates them, we can see that, given that mechanism, heating must be accompanied by light. 'Must' here means just what it means in the statement 'If you press the starter the car *must* start', i.e. 'If you press the starter the car *will* start, *provided* that everything else required has been done and the machinery is in order'. Now clearly the relations between the terms 'heat' and 'light' are not abstract formal relations, but intensional relations, because they depend upon what 'heat' and 'light' mean to a scientist. They do not depend upon some abstract formal schema into which the terms 'heat' and 'light' can be substituted for letters like '*p*' and '*q*'. When we grasp that heat and light are both forms of energy and that the atoms of substances are under certain circumstances treatable as machines which can transform the one into the other, we have got all the guarantee for the regularity that we could wish to have. No more examples of heat being accompanied by light are required after the appropriate intensional relations are established between the two terms. As Bacon put it we have scientific knowledge of a general kind when we have found the Forms of Simple Natures.

That this is the 'logic' of the guarantee of regularities can be strengthened by two more examples.

#### *Example B*

'Why is it that when zinc reacts with muriatic acid solution hydrogen gas is released on the surface of the zinc, and not, say, nitrogen or some other gas?'

*Answer* (at the level of school chemistry) -

Zinc goes into solution as positive ions. Muriatic acid is a compound of hydrogen and chlorine which dissociates in solution into hydrogen and chlorine ions. (To have called the acid 'hydrochloric acid' straight away gives the game away too easily, since that name expresses the discovery of the reason for the regularity). The zinc metal acquires a net negative charge, that is has a surplus of electrons. Hydrogen ions, having a net deficit of electrons take up the surplus electrons on the zinc metal, thus forming hydrogen atoms and



eventually hydrogen molecules which appear on the surface of the zinc as bubbles of hydrogen gas

Of course no other gas *can* be released in this reaction

(i) Since there are only zinc, hydrogen, chlorine and oxygen present in the experiment no other gas than hydrogen, chlorine or oxygen could be released

(ii) Given the ionic mechanism of the reaction and the atomic structure of the substances neither oxygen nor chlorine could exist as positive ions, hence neither could be released by the electron surplus of the zinc. So by knowing what the various reactants are, that is what their internal structures are, and what the mechanism of chemical reactions in solution is, we guarantee our belief in the regularity.

### *Example C*

'Of all children born to brown eyed parents, each of whom had a blue eyed parent, roughly 1 in 4 will be blue eyed.'

To guarantee this generalization as a regularity we need, not some abstract logical schema to relate observed family traits to the generalization, but an account of dominant and recessive genes and the mechanism of Mendelian inheritance

This last example brings out one final point of importance about the grounds for generalizing. In the beginning of a scientific investigation we know some of the regularities and are searching for a mechanism. To find the mechanism we need tentatively to hold to the truth of the initial generalizations. But the mechanism once found, or even sometimes once invented, we can use the mechanism itself as the guarantor of the generalized regularities. If you ask a scientist why he believes that such and such a phenomenon is a regularity he does not reel off the experimental evidence, and produce a logical and formal principle which binds that evidence to the generalization of the regularity, boldly facing the charge of affirming the consequent, as Kepler recommended he should do, but instead describes the mechanism by which the regularity is generated

R. HARRÉ

## VIII.—NEW BOOKS

*Evolution and Progress. Essays in Sociology and Social Philosophy.*  
Vol. III By MORRIS GINSBERG Heinemann. Pp. xii + 283.  
25s.

THIS is a wise man's book. Professor Ginsberg has lived through something of a revolution in sociology as well as through the "revolution in philosophy" to which he refers in his Introduction; he is at home in the literature of the nineteenth and early twentieth century founding fathers of the subject, British and continental, and he is able to take a long view. His own dominant interest is in social development, and its relation to moral progress. This is the main theme of these papers (mainly occasional, though the concluding chapters appear to have been specially written for this volume). The general title is "Evolution and Progress", but "development" is a more recurrent theme than either of these, and indeed forms a link between them. To examine the overlaps and distinctions between these notions is well worth doing, especially when we are faced, and not only from Sir Julian Huxley, with talk about "psycho-social evolution" and its possible continuity with biological evolution. Professor Ginsberg doubts whether it is helpful to carry over evolutionary notions from biology into the social sciences. He prefers to keep the term "evolution" to stand for the differentiation of species from a common ancestor, and the survival of some of these through natural selection (the latter being a hypothesis about the mechanism of evolution rather than part of its meaning). He finds this notion helpful in considering linguistic changes, and perhaps in some kinds of technological change. But changes in human society are also due to intelligence, willed purpose, and the preservation of social inheritance through customs, traditions, writing etc., so even if we still use the term "evolution" the mechanisms are very different from those of genetic evolution. Whether we speak of "social evolution" or not is surely a matter for recommendation rather than fiat; what is not optional, however, is to note the very great differences between the means whereby societies change and whereby species evolve. Professor Ginsberg's own preference is for the term "development". He takes this in general to mean "the realization of potentialities" (p. 29), or "the unfolding of man's powers, individual and collective". These well-worn phrases surely need more elucidation. In one sense it is surely trivial to say that if I do X, I must have been capable of doing X. In a non-trivial sense, there can be the appearance under favourable conditions of something which was there already in a different form (as in the "development" of a photographic film). What about uses which depend on the concealed analogy of "unfolding"? Can we seriously believe (any more than Professor Ginsberg in fact believes) that future stages of social processes, or institutions such as religion, marriage, or property, are somehow wound up in their early stages, waiting to be unwound by an Aristotelian actualization of their "ends", or a Hegelian immanent logic, or a kind of biological ontogenesis, as in the growth of a plant from a seed? Rather, to speak of social development as "the realization of potentialities" may be to say something like that a process has a recognizable continuity, such that later richer and more complex stages can be seen to have been reached through going through earlier simpler stages. (This is not saying that it could only have gone that way.) In Professor Ginsberg's usage, "development" is sometimes distinguished from progress, which is "development

or evolution in a direction which satisfies rational criteria of value" (p 30) But at other times "development" itself is used with reference to certain criteria, such as the growth of knowledge and control of the physical world, the capacity in people for detached and self-critical judgment, and growth in tendencies making for the unification of society on a world-wide scale "Development" is thus used with normative overtones, and thus, I think, is probably as it should be in such contexts "Progress" then becomes a more inclusive term, standing for the development of different capacities in relation to each other (e.g. technological skills and moral controls) along with appropriately adaptable institutions

But if "progress" is an all along the line notion of development, with the accent on morality, if "development" has in fact become a normative notion, and if "evolution" is doubtfully applicable to social processes, what are we to call those which do not measure up to our normative criteria? Presumably, just *changes* Professor Ginsberg does not say much about change, as distinct from development Chapter IV on "Social Change" (the Herbert Spencer lecture for 1958) is mainly an account of various views of social causation and social teleology, and ends with the belief that history points "to a series of groping efforts of men slowly becoming aware of their common needs and the possibilities of harmonious co-operation" (p 174)

Professor Ginsberg's sociological interest is manifestly related to a deep ethical concern Thus need not bias his presentation of sociological data, it means rather that he is asking how far sociological data give us any reason to think that we are gradually getting any nearer to achieving the conditions of a universal, rational, and moral society (He is aware that there are problems here over what is meant by "rational", and he has his views on them) But what a complicated business progress is, if social processes contain a number of different trends developing simultaneously, and not always to mutual advantage! Professor Ginsberg says (p 3) that for de Tocqueville "progress consisted in the movement towards equality" True, de Tocqueville believed that equality was on the way in, whether people liked it or not, and he used high language about Providence in saying this But he also believed that trends towards equality came at a cost: "the nation taken as a whole will be less brilliant, less glorious, and perhaps less strong". He spoke of "the progress of equality", but not (I think) of equality as progress *tout court* He saw that in social changes we have to take the rough with the smooth, in that trends of which we approve can bring undesired side effects, and a fresh train of problems Perhaps one of the main things sociological study can do for us is to make us better able to diagnose the unintended consequences and by-products of actions Professor Ginsberg is less interested in these than in tracing evidences for the increase in rational and purposive moral control And here, soberly and judiciously, he gives us reasons for hope

I have noticed one misprint. "much" on page 157, line 8, should read "must".

DOROTHY EMMET

*The Relevance of Whitehead* Philosophical Essays in Commemoration of the Centenary of the Birth of Alfred North Whitehead Edited by IVOR LECLERC George Allen & Unwin. Pp 383 42s

THIS volume contains fourteen essays by various contributors Eight of those represented work in the United States and Canada, four in Gt Britain

and two in West Germany. This suggests decreasing relevance as one moves East from the United States. The editor, Ivor Leclerc, states that the essayists were left free "to exemplify the relevance of Whitehead to their own thought and to the topic chosen by each". The only attempt to relate Whitehead's thinking to the contemporary philosophical situation in general is made by Charles Hartshorne in an introductory essay. He notes the recent publication of four expository books on Whitehead as indicating a revival of interest produced, according to Hartshorne, against a tide of positivism, pluralism, materialism, the doctrine of external relations and so forth. He concludes that most rejections of Whitehead are dogmatic and that his metaphysical speculations are still *sub judice*. The essay shows little appreciation of contemporary philosophy and gratuitously assumes that many philosophers have "rejected" Whitehead where it is more probably the case that they have simply bypassed his metaphysics.

I shall proceed now by summarising the remaining essays. William A. Christian makes the point that "religious enquiry" necessarily involves raising "speculative questions" and *vice versa*. This may be news to some contemporary theologians and philosophers.

Frederick B. Fitch offers, in ten pages, " . . . a brief sketch . . . of the nature of the universe, depicting the world in its spatio-temporal, epistemological and evaluative aspects". Charles Hartshorne discusses the modality of existential judgements and applies his results to the ontological argument for the existence of God holding that all it establishes is that some being but no particular being exists necessarily. The traditional conclusion " . . . and thus we call God " is not licensed by the argument.

A. H. Johnson contributes a useful and interesting survey of Whitehead's various scattered *dicta* on language and symbolism. Nathaniel Lawrence, with some obscurity, defends the view that the past can change by developing a theory of time on the basis of the fact that past events are often re-interpreted with reference to subsequent events. The thesis seems to confuse re-interpretation of the past with the reversibility of time.

Ivor Leclerc in a condensed discussion of different philosophical theories of matter and form manages to make clear the close kinship of Whitehead's view on this topic with Aristotle rather than with Plato.

Victor Lowe explains what he takes to be involved in Whitehead's characterisation of metaphysics as "the effort after the general characterisation of the world around us". The question of whether the effort is ever likely to succeed and under what restrictions and conditions it will succeed is never taken up.

Gottfried Martin sketches two ways of regarding metaphysics. One sees it as the science of principles the other as general ontology. Roughly, it is the difference between Aristotle's discussion of the law of non-contradiction and his investigation of the kind of being that the Ideas of Plato might have. The distinction appears to have some hermeneutic value but it requires more elaboration than Martin gives it here.

Wolfe Mays expounds Whitehead's 1906 article "On Mathematical Concepts of the Material World" in order to illustrate how Whitehead's speculative and scientific interests were always closely allied. The pioneering nature of this early application of formal symbolism to empirical materials is clearly brought out in Mays's exposition.

Eva Schaper claims that Whitehead's general theory of experience is able to make good sense of aesthetic experience for mainly two reasons. The first is that it rejects any kind of dualism of the mental/physical

variety or the feeling-perception kind and the second is the rejection of sensationalism as involving a false abstraction. These rejections lead on to an account of perception as "felt enjoyments" of things. One is tempted to remark that Whitehead's account of experience is an aesthetic view of perception rather than a perceptive view of aesthetics. Hermann Wein, in one of the most interesting contributions, deals with "the humanism of scientific procedure". He rejects the Marxist and Heideggerian doctrine of science as "technical appropriation of the world" and expands, mainly with reference to Kant, "the deeply serious conception of the knowledge of nature as a questioning by the judicial reason of man". The essay is recommended to students of K. R. Popper and M. Polanyi. Paul Weiss presents a cryptic account in his own terminology of the inferences that historians make from present data to past happenings. It is offered as an elucidation of certain passages in Whitehead cited by Weiss. It is difficult to decide whether Weiss's own statements or the citations from Whitehead are the more obscure and the whole proceeding seems to make obscurity doubly obscure.

W. P. D. Wightman deals with Whitehead's critique of traditional empiricism as "a solipsism of the present moment" due to a vicious abstraction from actual perception which neglects the durational aspect of experience, the feeling tone possessed in some degree by all experiences and the fact that elemental stretches of experience refer in some way beyond themselves, i.e. have a significance. The metaphysical implications of this are brought out and throw some light on Whitehead's difficult doctrine of the perishing of actual occasions. Finally, D. D. Williams discusses Whitehead's criticism of the Christian conception of God as the divine monarch and in particular Whitehead's charge that Christian theology has never made intelligible the precise mode of God's action upon the world. Inevitably Whitehead's own solution of the problem invokes his own heterodox conception of deity and hence will not recommend itself to orthodox Christian theologians. Despite this there is much of speculative religious interest in his doctrine.

It will be gathered from the above summaries that the essays are very largely contributed by unrepentant speculative metaphysicians. The most useful are those that deliberately set out to expound Whitehead such as those of Mays and Williams. Surprisingly none of the contributors chooses to expound Whitehead's considerable and widely influential contributions to modern logic. Too many of the pieces come under the heading of self-exposition. All this points to a failure of editorial policy. We gather from the mere fact of inclusion that Whitehead is somehow relevant to each of the writers concerned but we are not always clear as to the how and the why of it. Those of us who have read him cursorily—and it is worth remembering what good reading he can be despite sporadic metaphysical obscurity—suspect he is relevant to a good many matters of contemporary philosophical interest. Especially can a study of Whitehead constitute a test case in the perennial legal battle over the rights of metaphysics. Also current controversies over the relations of science and philosophy would benefit from a study of their incarnation in Whitehead. A less anarchic and more restrictive editorial policy designed to spotlight these areas would have celebrated a worthy occasion in a more useful fashion.

*Dizionario di Filosofia* By N. ABBAGNANO Turin: Unione tipografico-  
editrice Torinese Pp. xii+ 905 Lire 9000.

It is correct to say that this Dictionary is by Professor Abbagnano and not merely edited by him, for it appears from the Preface that with the exception of certain technical articles on logical topics the whole work was written by himself, though friends were consulted. This is an immense feat of erudition, for Abbagnano seems to be equally at home in references to ancient Greek, medieval scholastic and modern philosophy, and he refers as readily to the latest numbers of American journals as to the works of Plato. The subjects of the articles are well-chosen, and the articles themselves are informatively and dispassionately written. At the head of each article the Greek, Latin, English, French and German synonyms of the Italian term are given when they exist, this is very useful and very well done, though very many of the Greek terms have been misprinted. Serious defects are remarkably rare; under *Funzione Proposizionale* " $x - y$ " is said to be a propositional function, the article on *Induzione* fails to bring out the difference between Aristotle's and modern meanings of 'induction', the second meaning given to '*reale*', as the opposite of '*fittizio*', is a great over-simplification and the article on *Linguaggio* (p. 524, column 2) makes Russell say that a perfect language would have syntax but no vocabulary. Less seriously the article on *Costrutto* fails to do justice to the logical and non-metaphysical aspect of the doctrine of logical constructions, the Matrix method is assigned to Wittgenstein alone, the article on *Idea* suggests that Descartes first transformed the "*oggetto interno*" of divine thought into that of human thought, whereas the development of the term, both in philosophy and in the vernacular, had already done this for him, and Dummett is made an American (*Costruzionismo*). Other readers will no doubt notice similar points but they can be confident that the general level of accuracy and scholarship is very high. Abbagnano is particularly good at sorting out the different meanings given to terms, both consciously and unconsciously, at different times and places. There are no articles on individual philosophers, but there are articles with such titles as *Hegelismo* and *Platonismo*. Though the articles are all appropriate to a philosophical dictionary, Abbagnano includes such subjects as *Economia Politica*, *Genetica*, *Nestorianismo* and *Romanticismo*. Remarkably little concession is made to the more parochial interests of the Italian nation, for whom this dictionary is presumably in the first instance designed.

J. O. URNISON

*The Concise Encyclopaedia of Western Philosophy and Philosophers.* Edited by J. O. URNISON Hutchinson, 1960. Pp. 431. 50s

THE Editor explains in the Introduction the purpose of the Encyclopaedia and informs us to whom it is addressed. It aims primarily to give guidance to "the intelligent layman", but also to interest "the most advanced expert". It offers (a) guidance on the use of technical terms e.g. "analytic", "category", "universal", (b) descriptions of well-known "isms" e.g. "Realism", "Phenomenalism", "Empiricism", (c) accounts of individual philosophers, and (d) general descriptions of the main fields of philosophical inquiry, e.g. Ethics, Logic, Metaphysics.

The range of work is, therefore, immense: there are articles on all major and many minor philosophers from the Pre-Socratics to S. E. Toulmin, the youngest to be included. The articles are written by about fifty contributors, mostly British with a few from America, though the authorship of individual articles is not disclosed, they vary in length from fifty words on Urban to 6,000 on Kant. The work aims at being up-to-date and intercontinental. About thirty living philosophers are included, divided into thirteen English, twelve American and five European. Nearly a quarter of the available space is devoted to over a hundred plates, including eight in colour, many of them being photographs of living philosophers taken in a variety of surroundings, e.g. Sir David Ross at an Oxford reception and Nagel in what seems to be a public restaurant. The work ends with a bibliography, partly of histories of philosophy and partly lists of the principal works of the philosophers mentioned in the text. It is, in the main, adequate though Passmore's *100 Years of Philosophy* should have been included and the Editor's *Philosophical Analysis*. The only serious complaint one could make in this sphere concerns the omission of any reference to the many articles which are highly commended in the text and which the intelligent layman may well be at a complete loss to trace.

The Encyclopaedia has many successes and some failures. The articles of the last three groups indicated above are, with a few exceptions, extremely well done. The emphasis throughout is on giving some indication of the types of problems which have exercised philosophers together with some of the main answers: in addition an attempt is made to isolate those problems which chiefly concern contemporary thinkers. Everywhere "authoritative solutions" are studiously avoided. Particularly good are the articles on *Ethics* and *Metaphysics*, the latter being the outstanding article in the book. The liveliest is the one on *Epistemology*. The intelligent layman, having been soberly informed in the articles on *Rationalism* and *Empiricism* of the grave issues over which the rival contestants have battled for so long is here shown that "their tug-of-war lacks a rope". He may well be baffled, but he cannot fail to learn from this sparkling article. Aesthetics, and to a lesser degree Politics, receive as always meagre treatment.

The accounts of individual philosophers, with the exception of recent ones, are generally good, the best being those most difficult to write, e.g. Kant and the Greeks. Recent thinkers, with the exception of Russell, Moore and Wittgenstein, are so inadequately treated that one must query the wisdom of including them at all. For example, the article on Professor Price devotes two sentences to *Perception* and one to *Thinking and Experience*, an unique feat of compression. Jaspers, whose influence on English philosophers is said to be nil, is given about four times as much space as Austin whose influence is said to be considerable. There are also some inconsistencies e.g. Ayer, in the article on *Phenomenalism* is described as its "most unwavering supporter", and in the biographical note is said to have forsaken phenomenism. The choice of living philosophers may well appear eccentric to some, e.g. no room is found for Hampshire, Kneale and Farrer.

The weakest section is that on technical terms. These are interpreted so narrowly that less than twenty qualify. It was disconcerting to find that of the ten topics, chosen at random as likely to interest the intelligent layman, not one was included, viz. Action, Error, Self, Imagination, Memory, Space, Time, Causation, Belief, Necessity. The omission of most of

the traditional topics of Philosophy is defended by the Editor on the strange grounds that their inclusion would involve giving either authoritative or inconsistent answers. If, however, the Editor could trust his contributors to avoid partisanship over "isms", he could surely do so here; in any case a few such topics are admitted, e.g. Truth and Freedom of the Will. Many of the topics mentioned are naturally discussed here and there under "isms" or under the biographical headings, but the reader is given no help whatsoever to find them; hence this main criticism could largely have been forestalled had there been adequate cross-references or even an index. As it is, one has the impression that justice has been done more to Philosophers than to Philosophy. If it is impossible in the compass of 300 pages to do justice to both, then let the practitioners suffer rather than the subject.

It would be unjust to end on a carping note. One must sympathize with the Editor on the magnitude of his task and congratulate him on the remarkable lucidity, accuracy and interest of most of the articles and on the originality and freshness of many. His hope to interest and instruct both the layman and the expert will largely be fulfilled.

J. L. EVANS

*Philosophy of the Person* By P. A. MINKUS. Basil Blackwell, Oxford 1960. Pp. xv + 95. 12s. 6d.

THIS monograph is mainly an effort against a collection of possible sceptical attitudes to the identity of Persons. Gratitude to A. J. T. D. Wisdom is expressed in the preface along with the desire to understand "why people get spoken of as what they are not and to find sober sayings which will help us to appreciate what a person is, to see and see again how people are what they are".

This leaves us hopeful for we are to see a most permissive guidance of the tools of a critical analysis, the permissiveness of Wisdom, worked upon a most recalcitrant of philosophical difficulties, that of scepticism in the identity of Persons.

In the first chapter relevant doctrines of Locke, Hume and Reid are usefully discussed, in the second are seven aspects of 'The Scepticism As To Whether My Identity Remains The Same From This Moment To The Next Moment', while the last chapter of five pages, asks and answers, "What Is A Philosophic Question?" and "What Is A Philosophic Statement?"

There is no padding in this short work. Each sentence, crabbled or not, contributes something to the argument while sometimes an expository point is most happily put (14, 15, 16).

The chiding of scepticism runs as follows. Sceptical error in philosophy is often illuminating (68), though misdirected, so that philosophical correction often consists in just re-directing attention. The sceptic's "virtue is in the crime that his distinction is" (15).

The sceptic often has the facts but he has got them in such perverse arrangement that his argument has the form of *non sequitur* (64, 67, 69, etc.). Take the sceptic who says (63). "I can never know what the last feeling I appeared to feel was felt by for I no longer feel it. The consciousness-facts of times other than the present . . . are forever to be guessed at, for the identity with the present consciousness of a consciousness"



feeling at another time is forever to be guessed at, however rich the premises granted for the identity assertion, as long as they don't cease to be premises by begging the question of the identity." In short, I never am granted a perception of the unity of two temporally differing consciousness-facts and thus my use and re-use of "I" is "separate and apart and not for knowing."

This aspect of the systematically ambiguous "I" is re-directed by Minkus in one paragraph (64). Expectations and memories of consciousness-facts have one logic while premises and indirect tests as to their truth have a different logic. Having seen, say, is not proving that you have seen. And because this first sort of logic is different, "this leads the sceptic to say [there] is no knowing [the unity of my feelings]. But as all [the sceptic's] reflections about the differences are true and as we do not on the basis of these differences draw a distinction between 'knowing' and 'not knowing' his step must be a *non sequitur*."

But surely it is a shallow psychoanalysis which always sees in our scepticism, even those which are crimes, the same unwary genre of mistake. By now we ought to be bored with sceptics whose correction (61) "makes it to the point for us to remind [them] that the several senses of the general concept 'knowledge' are very different from each other . . .". We ought to be bored with sceptics who would be satisfied with the judgment that their arguments are *non sequiturs* because they have fouled the distinction between what is available to us now and what already has been or will be available to us (64, 66). Nor ought it to be easy to work up interest in sceptics who could learn from the argument of "the best and hence the normal use of language" (59). Such moves against such sceptics no longer get us the psycho-philosophical insights. Sceptics now have more sophisticated obsessions, obsessions which may even turn out to be articulate.

Perhaps you should read the last chapter first. Then it might be clearer why we meet only the sceptic whose "virtue is in the crime that his distinction is." In the last chapter we are told that a philosophical question is "a standing question as to nothing for thoughtful persons", is "a question that is habitually a challenge to clear thought", "a challenge, of saying 'the enemy!'" (91). Here it is clearest what manner of backwater we are visiting—one where the only good Indian is still a dead Indian. You might feel that before the correction of certain scepticism will yield us sharp insight into Persons, we must "understand why sceptics get spoken of as what they are not and . . . find sober sayings which will help us appreciate what a sceptic is, to see and see again how sceptics are what they are".

S COVAL

*La Métaphysique et le Langage* By LOUIS ROUGIER Paris: Flammarion, 1960. Pp. 252. 950 frs—T L

M. ROUGIER approaches the difficult problem of the relation between language and metaphysics in a somewhat disconcerting manner. What interests him is not what metaphysicians do to ordinary language, but, rather, language as a determining factor in the origin of metaphysics. The result is that he tends to treat the various metaphysical doctrines as anthropological phenomena rather than as contributions to philosophy.

The aim of the book, according to the author, is to show how the origin of Plato's and Aristotle's metaphysics lies in certain peculiarities of the Greek language, and the subsequent development of these theories by the Scholastics. There are chapters on the nominalist and empiricist reaction to Greek metaphysics in both the Middle Ages and modern times. Finally, with some remarks on the German language, he pursues this tradition in the essentialism of Husserl and the existentialism of Heidegger. The author concludes with a rather vague chapter on the value of metaphysics. He also expresses the hope that this work will incite the young to investigate the largely unexplored area that lies at the meeting point of several disciplines, *viz* logic, linguistics, history of religion, and history of philosophy.

It seems to me that M. Rougier does not succeed either in establishing his main thesis about the origin of Greek metaphysics or in vindicating his claim that there is a genuine inquiry of the sort he suggests. The results he obtains from his logico-positivist analysis of language are not clearly connected with the observations made from a linguist's point of view. On the other hand the historical treatment only shows that there is a broadly realist tradition in Western philosophy as well as a reaction to it, it does not bring out how linguistic peculiarities or idiosyncrasies give rise to metaphysics.

M. Rougier's book, though it contains some interesting suggestions, suffers from two main weaknesses both resulting from insufficient examination of the question of the relation of thought to language:

(a) His lack of interest in the worries of the metaphysicians together with the positivist view that ordinary language is illogical and badly constructed in contrast to science, prevents him from making clear how exactly the "illogicality" of ordinary language gives birth to metaphysics. Is it because Plato and Aristotle tried to do, using ordinary language, what physicists, logicians and mathematicians achieve by means of formal notations? In what way are linguistic forms, like or unlike notations, the result of thought? Surely these are the crucial questions yet M. Rougier passes them by. It is rather surprising then to find him saying that Aristotle went wrong in not recognising that universal propositions are hypothetical in character, and that there is a need for an existential quantifier, simply because he disregarded ordinary language (p. 40). It is also odd to find on page 51 the remark that the ambiguities of the definite article can be eliminated by the introduction of quantifiers one of which is "(x) = un certain x tel que . . .".

(b) M. Rougier formulates the problem about the relation of language to thought as follows: Does thought shape language or do linguistic forms condition thought? Without considering whether these are in fact mutually exclusive alternatives, he adopts the latter following what he calls the "linguists". Thus equipped he goes on to argue that the definite article in Greek, allowing as it does the creation of nouns out of verbs and adjectives, induced the Theory of Ideas in Plato and the theory of essences in Aristotle. Plato and Aristotle, according to M. Rougier, were duped by Greek into creating mythologies. Unlike Mr. Geach who once said that Plato's theory was such as to be *applied* expressed by the hypostatizing use of the definite article, the present author seems committed to the dubious view that Plato's theory could not have *originated* in a language that lacked this idiom. I am not sure what this claim involves, nor what arguments would support it. M. Rougier seems satisfied to remark that in Chinese, *e.g.* one can do without deductive or inductive forms of reasoning.

"La logique chinoise est une logique de l'Ordre, une sorte d'Étiquette universelle" (p. 48). Would it not have been more to the point to examine the contrast between Chinese patterns of argument with those in French?

KIMON LYCOS

*Renaissance Concepts of Method*. By NEAL W. GILBERT. Columbia University Press, New York, 1960. Pp. xxvi + 255 48s net

At one time it was taken for granted that the interest in method shown by seventeenth-century thinkers such as Bacon, Galileo, and Descartes was a novelty due solely to their scientific curiosity and independence of mind. But more recently it has been suggested by Cassirer and others that this interest derived from discussions of method by scholars of the previous century. The author of this study in the history of ideas tells us in his preface that he became interested in the problem when working as a graduate student of philosophy at Columbia and decided that it was necessary to make a broader investigation of Renaissance literature on the subject. The result is a very thorough and competent survey of the field. At the end Professor Gilbert writes 'It should be clear that much of the earlier discussion did not even touch upon the method of gaining valid and useful knowledge, but concerned only the method of transmitting an "already established art", or of pursuing a successful course of study. Putting these methods aside, there remains, to be sure, a faint residue of philosophizing that seems to concern the method of science, and many descriptions of method that sound surprisingly modern, but these are only descriptions, not prescriptions.

We may take this, then, as our general conclusion concerning this whole movement of thought. So far as practical effect upon methods of scientific investigation goes, the result of these long and wordy controversies was minimal, although in a few fields the arts methodology laid the grounds for the revamping of techniques of exposition, for instance, in law or history. So far as theoretical effect goes, there is no question but that in reviving the subject of method and in studying the philosophical works of the ancients, these Renaissance scholars were setting up a demand for a method which was to be answered, in the form of philosophical programmes, in the next century, and were also furnishing some hints from classical thought which do not altogether disappear from view even though the seventeenth century did not care to mention or to read what their predecessors had written on the subject.'

The conclusion is not exciting, but it is eminently sensible and well supported by evidence. After an account of the ancient and medieval sources available to Renaissance writers on method, Gilbert surveys their uses of the word 'method' and the growth of the custom of writing about method in dialectical textbooks. His principal characters are Ramus and Zabarella, but he gives information about many others, with quotations ample enough to carry conviction. For most of the humanists a method was no more than an efficient and compendious way of presenting a subject. The age was full of would-be reformers of education, and hundreds of books were written to show how the liberal arts and some other traditional subjects of study could be imparted in brisk and businesslike fashion. But, as Gilbert remarks, some seventeenth-century teachers of medicine and philosophy viewed the humanist reforms of Vives and Ramus as a threat to their own cherished ideal of strict science, and were stimulated in

consequence to try to expound a 'scientific' (i.e. knowledge-producing) method which they thought they found in Aristotle, Galen, Proclus, the Greek commentators on Aristotle, and Averroes. Their programme was not forward-looking, but their talk of resolution and composition seems to have had some influence on Galileo. From the humanist side the most useful contribution to the development of natural science was not a theory of method but a revival of interest in mathematics. For aesthetic reasons scholars cultivated Platonism (sometimes in rather grotesque ways), and Platonism brought with it increased respect for geometry. Again the results can be seen in Galileo's writings.

To most readers of *MIND* it will not come as a surprise that the works of Plato and Aristotle are more nourishing to the intellect than those of Cicero and Ovid. But it would be unfair to suggest that Gilbert has merely proved the obvious. On the contrary, he has made a useful contribution to the intellectual history of a rather obscure period by giving a scholarly account of a lot of second-rate matter. I confess that I do not want to read any more of the work of the logicians of that age, but why should such a result be expected from a review of their writings? It is a mistake to suppose that intellectual history is valuable only if it reveals forgotten masterpieces, just as it is a mistake to suppose that archaeologists are wasting their time unless they uncover works of art fit for exhibition in the great national collections.

WILLIAM KNEALE

*What is Philosophy?* By ELMER SPRAGUE. New York, Oxford University Press, 1961. Pp. 139. \$1.60

So little is said about so much in this book that a reviewer cannot but ask himself what purpose such a book can serve. The declared aim is to give the beginner an over-all view of philosophy before he becomes involved. But I doubt whether the untrained student will get more than a confused and kaleidoscopic impression from a book which devotes only a few paragraphs to every aspect of philosophy from the Ten Commandments to Dialectical Materialism. However simple and clear the style such condensation of content must be unpalatable to the uninitiated. Further, though we would expect the text of such a book to be unimpeachable as far as it goes, for the less we say the less chance we have of saying anything false, I feel that it must, nevertheless, be detrimental, just as we can lie without saying what is false, may it not mislead by leaving so much unsaid? I cannot attempt to support an affirmative answer to this question here since it would involve writing a book many times the length of this one. Though it is almost bound to be misleading in this way, we have a right to expect what is said to be as little misleading as possible, here, this is not the case.

For instance, though Idealism is considered by many to be untenable, it is not quite as ridiculous as Sprague would have us suppose. In his exposition, he interprets the Idealist thesis that material objects do not exist as meaning that the objects of perception are ideas, and hence are all we know and the only things for which we can claim existence. He then refutes this by saying that it is just untrue that we perceive ideas, if by 'ideas' we mean 'sensations'. His main example is the retinal image of which it is certainly true to say that it is not the object of our own perceptions. Via vision we perceive material objects, not visual sensations,

hence "we have no good reason for saying that the world is nothing but ideas". I feel sure that this is a misrepresentation of Idealism for I do not think it essential for the Idealists to decide whether sensations are the objects of perception or not - the Idealist claims that since, on the theory of representative perception, the image is interposed between mind and object, we have no reason for claiming that the image is an image of anything, *i.e.* that the objects of perception exist. He may or may not claim that we are aware of sensations, for his thesis is simply that only sensations are given, hence whatever we perceive, we have no right to assert the existence of anything but sensation. The Idealist may say that though we do not perceive our own retinal images we may perceive smells; it does not matter to the Idealist whether objects or sensations are the objects of perception; either way only sensations are given and hence only they exist. In any case, if Sprague wished to defend his thesis that sensations are never the objects of perception, he would have to distinguish between smells as given sensations, though unperceived, and smells as perceived though not perceived sensations. This would amount to defining sensations as unperceivable, not to asserting that they are, as a matter of fact, not so. A further confusion enters in when he tries to make his thesis a matter of logic by equating perceiving with sensing, in which case, to deny his thesis would be to claim that we sense our sensations - to claim this leads to an infinite regress for we would need a further set of senses beyond those which are originally stimulated in order to sense the original sensations and so on.

Again, in his exposition of Empiricism, Sprague misleads by putting the emphasis in the wrong place. He says - "to convince an Empiricist that what you are talking about does, or might exist, you must convince him that he and other people besides yourself can, or might experience it. If your statements about what there is are to be meaningful to an Empiricist, he must know how *he*<sup>1</sup> might go about discovering the existence or non-existence of what *you*<sup>1</sup> are talking about". This suggests that being publicly sensible is a criterion of the meaningfulness of existence claims, and hence of what exists. This criterion is an ordinary criterion of the reality of material objects, *e.g.* the objects of hallucinations and mirages are not counted as real. But I think the Empiricist wishes to emphasise the connection between meaningfulness and verifiability. He is not so specifically concerned with the criteria of material objects. The Empiricist need say simply that if no one can verify the existence or non-existence of an entity then the claim that it exists is meaningless. In other words, it is not essential that more than one person should be able to verify its existence. Certainly, in this case, the existence claim will be meaningless to everyone else. So far from the criterion of publicity being stressed, it should strictly be denied by the Empiricist. For instance, the claim that there are fairies at the bottom of my garden, visible only to me, is meaningful to me since I can verify it, but on the common-sense criterion of reality *i.e.* the criterion of publicity, a distinction is made between what is meaningful and what is real.

Another unnecessarily misleading statement is Sprague's comparison of religion and philosophy. Of all the views of religion which he might have taken, he chooses to discuss a view very like that of the heretical Jansenists of the seventeenth century. I need hardly say that the comparison is not exactly fair or correct.

<sup>1</sup>My italics

In sum, this book merely tends to bear out my opinion that all such books are both useless and dangerous. It is contrary to the spirit of philosophy, which aims to exorcise the misleading elements from ordinary language, not to add to the sum of inaccurate, ambiguous, misleading statements.

D. M. WRIGHT

*The Basic Writings of Bertrand Russell.* Edited with an introduction by ROBERT E. EGNER and LESTER E. DENONN. George Allen & Unwin Ltd, 1961. Pp 736 42s

THE title of this book is in many ways misleading; these are not the *basic* writings of Lord Russell at all. I can't help it if Russell himself talks of bringing together "so just an epitome" of his writings; with the exception of under a hundred pages out of 700, most of those 100 pages concerned with logic and mathematics, no man could see why he has been so justly influential in philosophy from reading this book. I therefore cannot help feeling that the *Basic Writings* would have been a better book with 'Basic' dropped from the title and the 100 pages dropped from the body; to read some of Russell's best—the clear, incisive, uncluttered prose of the Preface to *Principia Mathematica*—beside the reformed Victorianism of his worst—"sex is a natural need, like food and drink"—serves, I think, no useful purpose. I therefore cannot recommend *The Basic Writings of Bertrand Russell* as a purchase for philosophers, it belongs on the shelves of libraries.

However, someone who likes bedside reading by a philosopher will find in this book an undemanding selection of writings, all quite short. There are some autobiographical pieces, one of Russell's short stories and a selection of essays, some matters about language, snippets from his mathematical writing, a few essays (they are no more) taken from his more serious philosophical works, bits on psychology, education, politics, economics, history, religion, science, current events and, of course, war in the Nuclear Age. Here is a monument to the breadth of Russell's interests; unfortunately, here also is ample illustration of the carelessness of much of Russell's writing since the great change came some time in the twenties.

To take just one example which would be of philosophical interest if it were better done: Russell's attitude towards present day 'linguistic philosophy'. He opens the door to linguistic problems (in 'Theory of Knowledge' from *My Philosophical Development*) by saying, "I realized in 1918 that I had not paid enough attention to 'meaning' and to linguistic problems generally". He adds that he had not at all apprehended their difficulty and complexity. Nearly ten years later he is still in the same mood. In 'Language' (from *An Outline of Philosophy*, 1927) he writes, "The subject of language is one which has not been studied with sufficient care", though his own treatment thereafter shows that he has not been very careful himself. But by 1957, in 'The Cult of "Common Usage"' (from *Portraits from Memory*), he has given up that interest in language and has not even bothered to find out what the "certain linguistic doctrine" he finds so objectionable is about. Mentioning that he will be accused of misrepresenting the position, he characterizes it as follows, "The doctrine as I understand it, consists in maintaining that the language of daily life, with words used in their ordinary meanings, suffices for philosophy—which has no need of technical terms or of changes of significance—common terms". It would, I think, be very hard to find a philosopher

today who holds this view as a *philosophical* position. Certainly now there is a *bias* against the use of technical terms (and a very good bias it is too), but this arises from the fact that philosophers understand linguistic problems and the ease of misusing technical terms far better than they did. Some philosophers perhaps think that technical terms are unnecessary. But few if any philosophers feel that if a technical term or a word used in other than its normal signification appears in a philosophical work, then that work (philosophically, logically) *must* be worthless. Certainly this is not a position held in general by "the most influential school of philosophy in Britain at the present day". Russell here is charging windmills.

*The Basic Writings* is well produced with few misprints and clear type. There is a useful list of Russell's principal works and a chronology of his life. There is no index, but I hardly think that important as the table of contents is very full and the passages very short. There is nothing new by Russell except a Preface and a second short passage on his changed attitude towards induction on pages 154-155.

JON WHEATLEY

*The Ascent of Life: A philosophical study of the theory of evolution* By  
T. A. GOUDGE Allen & Unwin, 1961 Pp 236 30s.

PROFESSOR GOUDGE has written a very good book. His summary of accepted selectionist theory includes mention of many points of scientific controversy (e.g. onto-mutation, phylogenesis, epigenesis), and is clearly intelligible without being grossly oversimplified. After a stimulating discussion of some of the logical and linguistic features of evolutionary theory, he asks whether the theory has metaphysical implications outside the particular fields with which it deals scientifically. He shows that words such as "direction", "progress", "purpose", may change meaning when transferred from within evolutionary theory to the process of evolution as a whole, and *a fortiori* to extra-biological or cosmic evolution, but they do not necessarily lose all *scientific* meaning when so transferred. Thus wider questions using such terms can often be approached by way of scientific evidence, and can sometimes be given a definite answer. We may wish to accept certain metaphysical positions regarding evolution as a whole which coincide with our general world-view (e.g. Evolution exhibits purposive design), but such "explanations" can in no way *compete* with scientific explanations, though they may appear more or less plausible in the light of future discoveries of evolutionary science. Two persons may agree scientifically while disagreeing metaphysically.

In his discussion of the logical features of selectionist theory Professor Goudge says: "A crossword puzzle is a more apt model than a deductive chain in the light of which to understand the explanatory function of selectionist theory" (p 124). Whereas the theoretical structure of physics consists of purely nomological, causal statements which allow us to make precise predictions, selectionist theory contains *both* nomological statements about recurrent phenomena (e.g. the laws of phylogenetic trends), and "historical" statements concerning individual, non-recurrent events (e.g. the first occupation of dry land by vertebrates). These latter are explanatory, and causal, yet non-nomological: they are not universal statements, nor do they allow us to make precise predictions. The only predictions an evolutionist can make with any certainty are negative ones. On page 77 he says: "Whenever a narrative explanation of an event in evolution

is called for, the event is not an instance of a kind, but is a singular occurrence, something which has happened just once and which cannot recur. It is, therefore, not material for any generalization or law. The same is true of its proposed explanation. What we seek to formulate is a temporal sequence of conditions which, taken as a whole, constitutes a unique sufficient condition of *that* event. This sequence will likewise never recur, though various elements of it may." This contrast between the systematic and historical types of explanation combined in selectionist theory is weakened by the admission (p. 76) that the latter are not completely independent of laws, for they only function against a background of "a vast array of prior information, assumptions, inductive generalizations, etc.", which general knowledge is not peculiar to any one explanation, and is therefore extrinsic to the structure of the narrative explanation itself. "If laws are involved, they are so trivial that we need not mention them and rarely even notice them." But what are the criteria of triviality? A law may be "trivial" because it is familiar, and yet require an explanation, which cannot be easily found. Is "All unsupported bodies fall to the ground" a trivial law? And is "Offspring resemble their parents" trivial? Professor Goudge, I think, would say so. Yet in so far as neo-Mendelian genetics and the research of, for instance, Crick and Watson give us an explanation of this law in terms of the underlying mechanisms of inheritance, it is essentially linked with the systematic part of evolutionary theory, and thus lends greater weight to those historical explanations which assume it. Professor Goudge gives us a useful distinction, serving also to contrast the physicist and the cosmologist, but it admits of varying degrees. Some historical explanations are more nearly nomological, more theory-loaded, than others. And is the evolutionist really committed to believing that an identical sequence of events *cannot* recur, even on another planet?

Though he discusses various neo-Lamarckian viewpoints, Professor Goudge says of Lysenko: "Since his opposition has been motivated by ideological rather than scientific considerations, it does not require discussion here", and—uncharacteristically—he gives no reference. A discussion of Lysenko's standpoint would surely have been of much use, both in clarifying the logical criteria which must be satisfied to build or assess any *scientific* evolutionary theory, and also in showing how such a theory may be taken to have metaphysical implications, which then affect its evaluation in what *should* be a purely scientific context. (This is well discussed in J. S. Huxley's "Soviet Genetics and World Science".)

I found five misprints: p. 53, l. 1—for "revolutionary" read "evolutionary", p. 97, l. 19—delete "of", p. 162, l. 8—for "development" read "developmental", p. 195, l. 24—for "casual" read "causal", p. 214, l. 34—for "Duouguis" read "Decugis".

MARGARET A. BODEN

*The Constitution of Liberty* By F. A. HAYEK Routledge & Kegan Paul Ltd., London, 1960. Pp. x + 570 45s

In this book Professor Hayek endeavours to work out in some detail what liberty within a society is, what factors in society tend to diminish or increase it, its advantages and disadvantages to the members of that society and several other less central matters such as the errors and inequities of Socialism.



How does one sum up such a book in a short notice? Let me say that it is absolutely magnificent, but it is not philosophy. Or anyway, it is not *hard working* philosophy, the way we try to do it now. I do not want to laud present fashions but there surely is a real advance in trying to go a little way certainly rather than a long way uncertainly. Hayek's sweep is enormous, he wants to go a very, very long way. So far does he want to go, in fact, that I would hate to try estimating the number of individual points he seeks to make; it must be hundreds if not thousands and exception could be taken to a great many of them. Thus, though the book is monumental, the space devoted to really very ticklish little problems is often not great, certainly not great enough. Again, the *basis* of his investigations is often obscure, that is, it is often doubtful whether he is making a logical or a scientific point.

But the book *is* magnificent. It is a reasoned statement of faith, a book about politics on the highest level and a very fine one. I heartily recommend it, if not as philosophy.

JON WHEATLEY

*John Locke Dal Razionalismo all' Illuminismo.* By CARLO AUGUSTO VIANO. Giulio Einaudi editore, 1960. Pp. 618. Lira 4000.

In discussing Locke's philosophy in its various aspects, the author of this book also defends a thesis, and while this is perhaps not altogether novel, his defence in part is. His aim is to clarify the nature of Locke's empiricism which in his view had two main features: (a) an endeavour to liberalize knowledge from the *a priori* and deductive constructions of Cartesianism and to include within its scope the experimental, historical, and social sciences; (b) an attempt to assign to each intellectual pursuit a method of control whereby to ascertain its proper conditions and to limit its aspirations within a clearly defined subject-matter. Since much of the critical aspect of Cartesianism together with its technique of philosophical analysis was kept alive by Locke as part of both these tendencies, Signor Viano—as is also indicated by the sub-title of his book—treats his whole theme as an illustration of the nature of transition from Cartesian rationalism to the doctrines and attitudes of the Age of Enlightenment. In particular, he tries to describe the passage from Locke's early metaphysical *Essays on the law of nature* to the sober, critical standpoint of his maturity.

This interpretation, though at times over-emphasized here, is helpful, especially if applied to Locke's epistemology. In the first place, the author maintains, rightly it would seem, that though Locke primarily offered a psychological account of the origin of ideas, he also argued, as part of a more strictly philosophical thesis, that all significant concepts must be analysable in terms of simple sense impressions and also testable (in Prof. Price's phrase, 'cashable'), at least indirectly, by means of repeated sensory observation; for only in this way could experience become an instrument of control. The phrases 'sense' and 'sensory' here refer not only to ordinary sense perception but also to what in Locke's terminology is 'reflexion', i.e. introspection, which he concedes "might properly enough be called *internal sense*". Secondly, a particular advantage of this empiricist doctrine, in the author's opinion, was that it could provide a basis for ethics and explain in what sense there is a dependence of value on facts. It is often asked what Locke's empiricism in moral theory amounts to but this question is never satisfactorily answered; consideration should therefore be given to the author's attempt to elucidate it. He

argues that, as moral ideas, on Locke's view, are relational and the relations involved always those between an action and a rule, any comparison between the two terms of the relationship presupposes something that renders these homogeneous and commensurable. He shows that this can only be a set of ultimately given, simple ideas of sense. In the light of this explanation it is easy to see how Locke's empiricism was bound to undermine the metaphysically inspired theory of obligation set forth in his *Essays on the law of nature*; in fact to what extent it made morals arbitrary and relative. After all, its thesis was that moral truths are found not by intuiting innate rational principles but by starting from simple ideas of sense, though these of course would only be their necessary, not sufficient conditions, clearly then moral truths could be neither certain nor deducible. It might be thought that with this empiricist thesis Locke's notion of a demonstrable ethics, with which he also sympathized, was plainly incompatible. Here Signor Viano's discussion is likewise original and interesting. His point is that this notion had different functions and applications in the development of Locke's doctrine, depending for instance on whether its basis was religious or conceived as a purely linguistic matter or construed as the result of an alleged affinity between morals and mathematics. Even so, Locke would still appear here to be a rationalist rather than an empiricist. And there are admittedly other *non-empiricist* doctrines which he embraced and which cannot readily be fitted into the author's main exegesis. (For these see Professor Ryle's *Tercentenary Address on Locke*, O.U.P., 1933, pp. 26-27.)

Indeed, it is by no means obvious whether the author's lines of interpretation can be uniformly applied, as he suggests, to all parts of Locke's philosophy. For not only is this inconsistent at times; it also comprises subject-matters such as politics or theology which admit of an empiricist thesis only in a very modified sense. To say, for instance, that Locke liberalized political thought by dissociating it from the *a priori* and the general and basing it on the recognition of prevailing social and historical conditions (as this permits one to ascertain the real limits of the compatibility of interests and the legitimacy of claims made in public life) tends to confuse two parts of politics which Locke kept quite distinct—political philosophy and political art or prudence. Passing to details, I noticed a contradiction between statements on pages 252 and 260 as to whether Locke's description of the state of nature is or is not diametrically opposed to Hobbes's. It must also be emphasized that references to Draft B of the *Essay* should be, *faute de mieux*, to the manuscript now available in the Bodleian rather than to Rand's edition which, as I have explained elsewhere, is hopelessly incorrect. For this reason, incidentally, it is also unfortunate that an Italian translation of this edition should have been made and published in 1948.

Of the dozen or so books on Locke written during the last ten years this is the third from Italy. The reason for the many new publications is of course ultimately the recently acquired MS. material in Oxford, from which Signor Viano himself has drawn largely. He is also the first to have made full use of the shorthand notes for his overall appraisal of Locke's philosophy. Something else to his credit might, I think, be mentioned here, namely that he has recently edited (for the first time and before this has been done in England) Locke's two early treatises on the civil magistrate, as a part of a volume which conveniently contains all the different Lockean writings on toleration.

W. VON LEYDEN

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## IX.—NOTES

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Entries should reach The Secretary, The Aristotelian Society, 30 Frith Street, London, W 1, by not later than 31st December, 1963.

April, 1962

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# M I N D

## A QUARTERLY REVIEW

### OF

## PSYCHOLOGY AND PHILOSOPHY



### I.—KNOWING AND PROMISING

BY JONATHAN HARRISON

#### PART I

#### INDEPENDENT TREATMENT OF THE SUBJECT

#### I

IN Part I of my paper, I shall be concerned only with the following types of sentence. I shall be concerned with sentences such as 'I know she will be late', said by Jones of Miss Robinson at time  $t_1$ , and sentences such as 'Jones knew she would be late', said by Smith of Jones at time  $t_2$ , and with sentences such as 'I promise not to be late', said by Miss Robinson at time  $t_1$ , and sentences such as 'She promised not to be late', said by Smith of Miss Robinson at time  $t_2$ .

I shall not be concerned with sentences where the verb 'know' (or 'knows' or 'knew') is not followed by a form of words such as 'she will be late' or 'that she will be late'. This means that I shall not be concerned with sentences where the word 'know' or 'knows' or 'knew' is followed by words or phrases such as 'his (or my) way', 'his (or my) onions', 'Birmingham', 'the Lord's Prayer', 'how to play contract bridge', 'how the internal combustion engine works',<sup>1</sup> 'where she is', 'why it's not going', 'when she will come', 'what the matter is', 'not to eat peas with my knife', 'the difference between margarine and

<sup>1</sup> 'Knowing how' must not be confused with 'knowing how to', though it is perhaps unnecessarily pedantic to point out that what is normally referred to as 'knowing how' would be more accurately described as 'knowing how to'. Knowing how the internal combustion engine works for example, probably does only involve knowing that certain things about the working of the internal combustion engine are true.



butter'. The difference between those occurrences of 'know' with which I shall, and those with which I shall not, be concerned, is that with the former, but not with the latter, the words following 'know' express something capable of being true or false. It makes sense to say 'It is true (or false) that she is late', but it does not make sense to say 'Birmingham is true (or false)' or 'How to play contract bridge is true (or false)'.

Nor shall I for the moment be concerned with those cases where the word 'promise' (or 'promises' or 'promised') is followed by words like 'that she will not be late'. I am not suggesting that there is necessarily an important difference between promising to and promising that, but there may be, and when I promise that something will happen, over which I have, and know I have, and am known by my hearers to have, no control, there almost certainly is such a difference. I cannot break or keep this promise.

## II

I think that, until fairly recently, most philosophers would have taken for granted the following things about what anyone who said 'I know she will be late' was doing.

(i) It would have been taken for granted that he was making a statement, or asserting a proposition, or saying something which could be true or false.

(ii) It would have been taken for granted that the proposition asserted by someone who said 'I know she will be late' was a different proposition from the one he would have asserted had he simply said 'She will be late'. It would have been taken for granted that if the former proposition were true, the latter proposition would have to be true too, but it would also have been taken for granted that the latter proposition could be true, but the former false.

(iii) It would have been taken for granted that when Jones said, at time  $t_1$ , 'I know she will be late', he was asserting the same proposition as that asserted by Smith, at time  $t_2$ , when he says 'Jones knew she would be late'. That is to say, it would have been taken for granted, not only that both Jones and Smith were saying something which had a truth value, but that what they were saying must have the same truth-value; that Jones could not be saying something true, and Smith be saying something false, and *vice versa*.

(iv) It would also have been taken for granted that at least one problem involved in 'the problem of knowledge' was to say just

what Jones was asserting about himself, when he said things like 'I know she will be late', and just what Smith was asserting about Jones, when he says 'Jones knew she would be late'. Briefly, it seemed natural to suppose that, since there were some true propositions which Jones knew, and others which he did not, the difference between his knowing some and not others lay in Jones; in what might, very inadequately, be called Jones's 'mental attitude' to these propositions, or his 'state of mind' concerning them. Again, since there were some true propositions which Jones knew, and others which he only believed, it seemed natural to suppose that the difference between Jones's knowing some true propositions, and only believing others, lay in Jones, again, in his 'state of mind' concerning them, or his 'mental attitude' towards them.

### III

If what Jones is doing when he says 'I know she will be late' is like what Miss Robinson is doing when she says 'I promise not to be late', then all these traditionally made assumptions are wrong.

(i) When Miss Robinson says 'I promise not to be late', she is not saying something which can be intelligibly said to be true, or intelligibly said to be false. If Miss Robinson were to say to Smith, 'I promise not to be late', it would be improper for him to reply, 'That's true' or 'That isn't true', 'I agree' or 'I don't agree' or 'You must be mistaken'. If Smith were to say any of these things, it would show that he had misheard, or failed to understand, what Miss Robinson had said. Hence, if 'I know . . .' is like 'I promise . . .', anyone saying 'I know . . .' will not be saying anything capable of being true or false, nor will he be capable of being mistaken.

(ii) If anyone saying 'I know she will be late' is not making a statement at all—and, if 'I know . . .' is like 'I promise . . .', he will not be—any question concerning the logical relations between the statement he is making, and the statement he would have been making if he had detached the words following 'know', viz. 'she will be late' from the whole sentence, and asserted them independently, does not arise. It is worth pointing out that it is natural to suppose that such a problem does arise in the case of 'I know . . .', though not at all natural to suppose it arises in the case of 'I promise . . .', for the words following 'know' could, in other contexts, express something capable of being true or false, but the words following 'promise' could not.

(iii) Since Miss Robinson, when she says at time  $t_1$ , 'I promise not to be late', is not making a statement, it is clear that she cannot be making the same statement that Smith makes at time  $t_2$  when he says, 'Miss Robinson promised not to be late'. Hence, if 'I know . . .' is like 'I promise . . .', Jones, when he says, at time  $t_1$ , 'I know she will be late', cannot be making the same statement that Smith is making when he says at time  $t_2$ , 'Jones knew she would be late'.

(iv) Again, since Miss Robinson, when she says 'I promise not to be late', is not making a statement, she cannot be making a statement about herself, and any one who supposed that this sentence asserted the occurrence of some 'mental act', which actually was the promising, which the sentence 'I promise not to be late' described, and tried to give an account of this act, would be chasing a will o' the wisp. Hence, anyone asking what the special 'state of mind' Jones must be in when he truly says 'I know she will be late' will, if 'I know . . .' is like 'I promise . . .', also be chasing a will-o'-the-wisp.

#### IV

What Miss Robinson is doing when she says 'I promise not to be late', however, differs very considerably from what Jones is doing when he says 'I know she will be late', and in the following ways.

(i) When Miss Robinson says 'I promise not to be late', she has committed herself to doing something, *i.e.* to arriving on time, but she has not committed herself to any proposition's being true. When Jones says 'I know she will be late', however, he has committed himself, at the very least, to the proposition 'She will be late'.

(ii) Hence, if it turns out that, after all, she is not late, Jones will have to admit that he was wrong. There is no future eventuality, however, which could make Miss Robinson say that what she said when she said 'I promise not to be late' was wrong. Saying that something is wrong, of course, must not be confused with saying that somebody is wrong to say that thing, it can be wrong—and even a criminal offence—to say many things which are not wrong, and it can certainly be wrong to say 'I promise . . .'.

(iii) From the fact that what Jones says when he says 'I know she will be late' can be wrong, I am inclined to infer that it can have a truth-value. It must take the truth-value 'false', if she is not in fact late. Indeed if Jones said 'I know she will be

late' when he knew perfectly well she would not be late, then what he said will not only be false, it will also very probably be the case that he is lying. Miss Robinson, on the other hand, when she says 'I promise not to be late' cannot be saying what is false, and cannot be lying. What is vulgarly called a 'lying promise' is not a lie, but a promise which one has no intention of keeping.

(iv) Can 'I know she will be late' be false, even if she is in fact late, as has always, until recently, been assumed? I am inclined to think it can, and for the following reason. Smith may perfectly well know that Miss Robinson was late, and yet not be convinced that Jones knew she would be, even though he is quite convinced that Jones said 'I know she will be late'. He may even be quite convinced that Jones did not know she would be late, even though Jones said 'I know she will be late', and she was late. It seems to me that, if Smith is right in thinking that Jones did not know she would be late when he said he did, even though she was late, then what Jones said must be wrong, and, if it was wrong, I am inclined to think it must have been wrong by being false. If this is the case, then what Jones is saying when he says 'I know she will be late' can be false, even though it turns out that she is late.

(v) The above argument, of course, assumes that what Smith says at time  $t_2$ , viz. 'Jones did not know she would be late', is incompatible with what Jones says at time  $t_1$ , viz. 'I know she will be late', for it reasons that since what Smith says is obviously capable of being true, even if she is in fact late, then what Jones says must be capable of being false in the same circumstances. This is just one of the very assumptions which will be invalid if 'I know . . .' is like 'I promise . . .', for what Miss Robinson says when she says 'I promise not to be late', is certainly not incompatible with what Smith says when he says 'Miss Robinson did not promise not to be late'. The fact that Miss Robinson has said this, of course, is a very strong reason—though not a conclusive reason, for she might, for example, have been joking—for thinking that Smith is wrong, but this does not mean that what she says is incompatible with what Smith says; at most the statement that she has said these words is incompatible with what Smith says. This is because one of the things which can make Smith's statement true is Miss Robinson's saying these words. One way—though not the only, or even the usual, way of promising—is to say 'I promise . . .' in certain standard circumstances, *e.g.* in a language where these words have this function conventionally allotted to them, without the speaker winking, with knowledge of

what he is saying, and understood by his hearer, etc. If these words have been said, and there is nothing to make saying them misfire, then Miss Robinson has promised. Here promising is not something over and above her saying these words, some mental act, for example, which these words are about, and the occurrence of which would make what she says true, and the non-occurrence of which would make what she says false. It is, as is sufficiently well recognised, just the saying of the words 'I promise . . .', and if she has said them, and the standard conditions are fulfilled, then Smith's statement 'Miss Robinson promised she would not be late' must be granted. But no-one could possibly maintain that what Smith says when he says 'Jones knew she would be late' is made true simply by the fact that Jones said the words 'I know she will be late'. One way of promising is just saying 'I promise . . .', but saying 'I know . . .' is not any way of knowing. Hence what Jones says when he says 'I know she will be late', and what Smith says when he says 'Jones knew she would be late' are not related in anything like the way in which what Miss Robinson says when she says 'I promise not to be late' is related to what Smith says when he says 'Miss Robinson promised not to be late'. This being so, there is no reason for supposing that they are not related in the way in which everybody, until recently, supposed they were related; no reason for supposing that what Jones and Smith are saying are not logically equivalent. If they are logically equivalent, then, since what Smith says when he says 'Jones knew Miss Robinson would be late' can be false, even though Miss Robinson was actually late, then what Jones says when he says 'I know she will be late' can also be false in the same circumstances, and what Jones says will not only have a truth-value, but a different truth-value from 'Miss Robinson will be late'.

(vi) If 'I know she will be late', said by Jones at time  $t_1$ , is capable of being true or false, and can be false independently of Miss Robinson's being late, it seems not unnatural to suppose that the reason for all this is that it is a statement about Jones, in the sense that at least one of the things relevant to its truth is something about Jones. Jones is, in fact, saying about himself precisely what Smith is saying about him when Smith says at time  $t_2$  'Jones knew Miss Robinson would be late'.

(vii) The arguments I have used do not show that, when Jones says 'I know Miss Robinson will be late', he is not engaging in a performance, over and above the performance of saying the words 'I know Miss Robinson will be late'. All it shows is that, if he is engaged in a performance, it is not a

performance which is described by Smith when he says 'Jones knew Miss Robinson would be late'. In this respect, then, saying 'I know . . . ' is unlike saying 'I promise . . . ', but it may nevertheless be like saying 'I promise . . . ' in that anyone saying it is engaged in a performance, though not a performance which the words 'He knew . . . ' describe or assert to have occurred. Jones, when he says 'I know . . . ', may be staking his reputation, licensing others to argue or behave in certain ways, giving others his authority for saying certain things. even though someone saying 'Jones knows . . . ' is not saying that he is doing any of these things I do not wish to deny that he is doing any of these things, though I doubt whether any of these descriptions of what he is doing are very helpful. But does it follow from the fact that he is doing these things that he is not doing what everyone, until recently, assumed he was doing. 12. making a statement about himself? It seems to me that this does not follow. To take examples from a different sphere, when I say that Thompson is a very good man, I may, I suppose, be described as commending him; when I say that a loaf is of the very best bread, I may, I suppose, be described as evaluating or appraising it, when I say that what Atkinson did was wrong, I may be condemning his action; when I say that Richardson is a bounder I may, to put it mildly, be described as expressing an unfavourable or con-attitude to him. It does not, however, follow from the fact that these descriptions of these men and this action are applicable, that what I am doing when I say these things does not consist in saying something capable of being true or false, any more than from the fact that what I am doing may consist in, say, reassuring someone, it would follow that I was not saying something capable of being true or false. Similarly, one way, though not the only way, of, say, staking one's reputation may be to make a certain statement about oneself. the statement that one knows such-and-such to be the case.

V

The upshot of this is that the suggestion that 'I know . . . ' is like 'I promise . . . ' is, at the very least, extremely misleading I would not personally like to say that there was no resemblance between them, but it seems clear that the differences are more obvious and more important than the resemblances. One of the most important differences is that someone saying 'I promise . . . ' is thereby promising. whereas someone saying 'I know . . . ' is not thereby knowing. but simply claiming that he knows.

Hence, though the question whether he really has promised may turn on the question whether he has said 'I promise . . .', and not on the question whether what he said when he said 'I promise . . .' is a true statement about himself, the question whether he really knows does not turn on whether he has said 'I know . . .', but on whether, having said 'I know . . .', he has said something about himself which is true. If this is so, then past philosophers have not been chasing a will-o'-the-wisp when they asked 'In what condition must someone be who correctly says that he knows something, for his claim that he knows to be justified?'—or, at any rate, one reason for thinking they have been doing this is seen to be invalid.

## VI

The above argument has been concerned with sentences like 'I promise not to be late', and not with sentences such as 'I promise that I will not be late'. Perhaps it would be as well to consider to what extent what has been said about 'promising to' applies to 'promising that', and to what extent it does not.

Sometimes one says 'I promise to . . .' when one might just as well have said 'I promise that . ..'. Whether Miss Robinson says 'I promise not to be late' or 'I promise that I will not be late', Smith can, in either case, say of her either 'Miss Robinson promised not to be late' or 'Miss Robinson promised that she would not be late'. In such cases I believe there is no important difference between 'promising to' and 'promising that', though in one respect 'promising that' resembles saying 'I know . . .' more closely than does 'promising to'; in both 'I promise I will not be late' and 'I know I will not be late' the words following 'promise' and 'know' respectively could, in other contexts, be used to assert something capable of being true or false. However, both 'promising to' and 'promising that' are essentially different from saying 'I know . . .', in that someone who says either 'I promise not to be late' or 'I promise that I will not be late' is correctly described as having promised, but someone saying 'I know . . .' is not correctly described as having known.

There are, however, cases when someone says 'I promise that . . .' when he could not just as well have said 'I promise to . ..'. Mrs. James can say 'I promise that Tommy will not be late', when it is not clear just how what she says could be paraphrased in terms of 'promising to'. Perhaps what she means is 'I promise not to let Tommy be late', but this is

dubious, for she may feel sufficiently confident of Tommy's punctuality to be able to promise that he will not be late, without feeling that it will be necessary for her to take any steps to see that Tommy is not late. Indeed, sometimes when someone says 'I promise that . . .' there are no steps which he could take, or could be taken to see that the eventuality promised occurs, as when, for example, the Astronomer Royal says 'I promise that there will be an eclipse of the sun at 21.07 tomorrow'. In such cases 'promising that' is increasingly different from 'promising to'. It is, nevertheless, still unlike saying 'I know . . .', for the question whether the Astronomer Royal promised there would be an eclipse is settled by a consideration of what he said, *i.e.*, of whether he said 'I promise that there will be an eclipse . . .', but the question whether or not he knew there would be an eclipse is not settled by a consideration of what he said.

## PART II

### PROFESSOR AUSTIN ON 'I KNOW' AND 'I PROMISE'

'Attacking men of straw' is an exercise which sometimes is philosophically enlightening. Since, however, there are some who have a prejudice against criticising views which have never been held, it may be as well to consider to what extent the late Professor J. L. Austin thought that 'I know . . .' resembles 'I promise . . .' in ways in which these two do not resemble one another.<sup>1</sup>

## I

Austin maintained that to suppose 'that "I know" is a descriptive phrase, is only one example of the *descriptive fallacy*, so common in philosophy' (p. 146). I am not sure what the 'descriptive fallacy' is, however. If the 'descriptive fallacy' is simply the fallacy of supposing that 'I know she will be late' is like 'She is habitually unpunctual', and unlike 'Twice two are four', 'Dodos no longer exist' or 'The Battle of Hastings was fought in 1066', describes something, then Austin would certainly be right, but I doubt whether this contention, though true, would be relevant and important, or was the one he wished to make. If, however, the 'descriptive fallacy' is the fallacy of supposing that the function of certain words is to make a statement, expressing some truth or falsehood, when they have some other

<sup>1</sup> *Logic and Language* (ed. A. G. N. Flew), 'Other Minds', pp. 142-147.



function, then my arguments do tend to show that Austin was wrong, and words like 'I know . . .' do express a statement.

## II

Perhaps the 'descriptive fallacy', however, lies in supposing, not that the sentences like 'I know p' do not convey something capable of being true or false, but that putting 'I know' in front of any sentence does not alter the truth-value of what is being said, much as putting 'I warn you that' in front of 'The bull is going to charge' does not alter the truth-value of what is being said. Austin does not say that this is the way in which the words 'I know' are not descriptive, but he does himself mention 'I warn', etc., very shortly after saying that 'I know' is not a descriptive phrase (p. 145), and it is possible that he did think that the function of 'I know' was like the function of 'I warn'. If he did think this, then he was mistaken, for, though 'I warn you that the bull is going to charge' does not have a different truth-value from 'The bull is going to charge', 'I know that the bull is going to charge' does have a different truth-value from 'The bull is going to charge'. When said by Jones, 'I know the bull is going to charge' has the same truth-value as 'Jones knows the bull is going to charge', said by Smith. The view that adding 'I know' to 'The bull is going to charge' does not alter the truth-value of what the latter asserts has been more explicitly held, if I am not mistaken, by Mr. Urmson.<sup>1</sup> The above argument seems to me clearly to dispose of it.

## III

It is clear that, if anyone were to suppose that the function of sentences like 'I promise . . .' was to make an assertion about the speaker, he would be making a very serious error, and an error which could quite appropriately be described as a descriptive fallacy. Austin, immediately after having remarked that to suppose 'I know' is a descriptive phrase is to commit the 'descriptive fallacy', mentions 'obvious ritual phrases' such as 'I do' as examples of the non-descriptive use of language; when we say 'I do' we are not describing the action, but doing it (p. 147). Since, clearly, when we say 'I promise . . .' we are also not describing the action, but doing it, and since Austin has likened 'I know' to 'I promise', it is at least possible that he thought 'I know' was non-descriptive in the way in which 'I promise' is non-descriptive. If he did think this, he was again mistaken, for, as I have

<sup>1</sup> 'Parenthetical Verbs', *MIND*, 1952.

already shown, though someone saying 'I promise' in the appropriate circumstances, is promising, not claiming to promise, someone saying 'I know' is simply claiming to know, not knowing. The temptation to the reader to suppose that Austin did think that 'I know' was like 'I promise' in this respect is very strong, so strong, indeed, that if he did not think so, it was quite extraordinary that he should not have pointed this out.

## IV

Austin himself points out one extremely important difference between 'I know' and 'I promise', viz. that someone who promises what he does not perform has, nevertheless, promised, though someone who says he knows something which is false cannot have known. He suggests, however, that this difference is 'more apparent than real. The sense in which you "did promise" is that you did say you promised (did say "I promise") : and you did say you knew' (p. 145). There is, however, according to Austin, another sense of 'promise' in which he who says 'I promise', but does not do what he promised, or did not fully intend to do what he promised, or promised what was not within his power, did not promise. In this sense of 'promise', promising and knowing are similar (145).

There is, however, only one sense of 'promise'; the only way of promising is to say 'I promise' (or use some other equivalent phrase or gesture) in the appropriate conditions. There is no sense in which someone, having said 'I promise' in these conditions, can be said, nevertheless, not to have promised. Hence there is no sense in which someone who promises what he does not intend, or what is outside his power, or who promises, but does not perform, has not promised. Indeed if there were, it would follow that there was a sense of 'promise' in which it was logically impossible to promise what you do not intend to, or what you cannot, or what you do not perform, from which it would follow that there was a sense of 'promise' in which it was logically impossible to break a promise, which is absurd. If you say 'I promise X' when you do not fully intend to do X, or cannot do X, or subsequently do not do X, then you may have been casual, inconsiderate, or immoral, but you have made a promise. Hence someone who claims to know something that is, in fact, false, only says 'I know', and does not in any sense know; someone who says 'I promise', but does not perform, not only says 'I promise', but promises, and does not in any sense not promise.

Strictly speaking, Austin does not say that there is a sense of 'promise' in which he who promises but does not perform, etc., has not promised, but that there is "a 'sense' of promise" in which this is true. I do not understand the unexplained difference between a sense of 'promise' and a 'sense' of promise, especially when the latter is contrasted with a sense of 'promise' (and not a 'sense' of promise) in which he who promises, but does not perform, has promised.

## V

Austin rightly suggests that sometimes the worry concerning whether Jones did promise, or order me, or marry his landlady, or warn me that the bull was about to charge, may be a worry concerning whether Jones, having said 'I promise' or 'order you' or 'I do' or 'I warn you', said these words in the appropriate circumstances, or whether, because he winked, or had no authority, or was not in front of a real clergyman, or knew nothing about bulls, the appropriate circumstances did not all obtain, and so, though Jones did say 'I promise', etc., Jones cannot properly be described as having promised. He then says "We hesitate between 'He didn't order me', 'He had no right to order me', 'He oughtn't to have said he ordered me', just as we do between 'You didn't know', 'You can't have known', 'You had no right to say you knew ...'" (145-146). There is an enormous difference, however, between our worry concerning whether someone did warn, or order, or promise, or marry, and our worry concerning whether he knew. Our worry concerning whether he did warn, or order, or promise, or marry, is a worry concerning whether he, having said 'I warn', or 'order' or 'I promise' or 'I do', said these in the appropriate circumstances. But North's worry concerning whether East knows he has the King of Spades is not a worry concerning whether East, having said 'I know North has the King of Spades', said this in the appropriate circumstances. East can perfectly well know this, without *saying* anything at all.

## VI

Austin suggests that if someone were to suppose that the difference between 'I promise' and 'I fully intend' was that promising is something higher in the same scale than fully intending, he would be mistaken. He also suggests that if someone thought that the difference between 'I know' and 'I believe' or 'I am certain' was that knowing was higher in the same scale than

believing, he would be mistaken. If all he means is that someone who knows is not more certain of what he knows than someone who is merely absolutely certain, then he is surely right. There is nothing more certain than certain. But he may be suggesting that, just as someone would be mistaken who supposed that 'I promise' was a different and 'more extreme' statement about the speaker than 'I fully intend', so someone who supposed that 'I know' makes a different and 'more extreme' statement about the speaker than 'I am quite sure' would also be mistaken. It is clear that—supposing that one who says 'I fully intend' is making a statement about himself—that to adopt the first course would be to make a mistake, and a bad one. The above arguments have shown, however, that to adopt the second course—supposing that one who says 'I am quite sure' is making a statement about himself—is not to make a mistake.

## VII

Austin claims 'But the essential factors are (a) You said you knew you said you promised, (b) You were mistaken: you didn't perform' (146). Here Austin has made knowing and promising appear more similar than they are, for he appears to suppose that 'You said you promised' parallels 'You said you knew'. Thus it does not. 'You said you knew' is indirect speech for 'You said "I know"', but 'You said you promised' is not indirect speech for 'You said "I promise"', but indirect speech for 'You said "I promised"'. Illegitimately substituting 'You said you promised' for 'You promised' or 'You said "I promise"', makes it possible for Austin to suggest that, just as you said you knew, but did not, because you were mistaken, so you said you promised, but could not have done, because you did not perform. But though you did not know what turned out to be false, you did promise what you did not perform, for all you need to do to promise is simply to say 'I promise' with a straight face, but a lot more is required to know than to say 'I know'.

## VIII

Though it is not always entirely clear in what ways Austin thinks 'I know' resembles 'I promise', he is quite explicit on one point. Both someone who says 'I know' and someone who says 'I promise' give others their word (144). There are, however, cases when we would certainly not say of someone who said 'I know p' that he gave his word that p. If X says to me

'I know that you are determined to make a fool of yourself', he is surely not correctly described as having given me his word, pledged his authority, that I will make a fool of myself. When the amateur investigator says to the detective-inspector 'I know there is strong circumstantial evidence against him, but . . . ' it would be very odd to describe what he is doing in the way Austin does. In general, when X says to Y 'I know that p', when he believes Y to be already aware of p, he is not doing this so much to assure Y of p, as to communicate to Y the fact that he also knows p. In such cases, to describe X as giving his word to Y that p is true would be thoroughly implausible.

It is true that we could perhaps say that when X says to Y 'I know that p' when Y himself is not aware of the fact that p, we could describe X as giving his word to Y that p, and describe the situation which obtains when Y already knows p differently. X, however, may say 'I know the gun is loaded' both to Y, who knows this too, and to Z, who does not, but wants to know whether it is or not. In this case, he *may* be giving his word that the gun is loaded to Z, who is concerned about this, but he certainly cannot be described as doing this when he says the same thing to Y, who knows this already, and is only interested in whether X, too, knows. Since he is, presumably, saying the same thing to Y that he is saying to Z, and he is making a statement to Y, it follows that he is also making a statement to Z. Hence, even if it is correct to describe him as giving his word to Z, this description is not incompatible with his making a statement about himself. Even if Austin were sometimes right in thinking that someone saying 'I know p' is giving his word, he who says 'I know p' is, nevertheless, always also making a statement.

## IX

But is Austin's description of what someone saying 'I know p' is doing ever true? Is it ever the case that someone saying 'I know p' is properly described as giving his word that p, etc.? I do not think that it is. Someone who says 'I give you my word that it isn't loaded', 'I guarantee it isn't loaded', 'I swear it isn't loaded', can properly be described as having given his word that it was not loaded, but someone who simply says 'I know it isn't loaded' cannot. He is doing, what the others are certainly not doing, stating a fact about himself. The effect of his stating this fact may be the same as the effect of his saying 'I swear the gun is not loaded', someone does, or does not, pull the trigger, depending upon what it is they wish to achieve.

Similarly, the purpose of someone who says 'I know the gun is loaded' may be the same as the purpose of someone who says 'I swear that the gun isn't loaded', viz. to cause someone to pull, or not to pull, the trigger, depending upon whether the speaker does, or does not, want the trigger pulled. This is because 'I know the gun is loaded' entails 'The gun is loaded' which may be what his hearer wants to know. But perhaps he already knows this, and wants to know whether the speaker knows this too. In this latter event, 'I swear the gun is loaded' would not do for giving Y the information he needs.

When the interesting and important discovery that language had other uses than to state facts, describe things, or communicate information was first made, it was only natural that philosophers should over-estimate the extent to which language was 'non-propositional' and the extent to which age-old philosophical problems had arisen as a result of the mistaken view that it was. The view that the function of 'I know' resembles the function of 'I promise' is a case in point. The discovery, largely due to Austin, that someone saying 'I promise' or 'I do' or 'I warn' or 'I name' or 'I take' was not making a statement about himself to the effect that he was in some special state, or undertaking some special performance, but actually, by saying these words, engaging in the performance, was both interesting and important. The assimilation of the more philosophically important words 'I know', however tentative or partial, to this class of words is just a mistake. Our predecessors, so far from committing the 'descriptive fallacy' with regard to 'I know', had the better of us in that they did not make this mistake.

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## II.—QUALITIES AND ILLUSIONS

BY ELIZABETH H. WOLGAST

THE argument from illusion arises from consideration of any of a large class of everyday phenomena.<sup>1</sup> They are things we learn early in life to describe, no one denies they exist. Yet from these examples philosophers have been led to assert that our concepts of material things, of qualities belonging to them, and of knowledge about them are self-contradictory.

I would like to discuss why the argument from illusion *seems* to yield such a conclusion. And I want to consider what it directly implies for a philosophy of the material world and our knowledge of it.

### I

Russell once used as an instance of illusion the changing appearance of a table as one walks around it. He says

A table viewed from one place presents a different appearance from that which it presents from another place. This is the language of common sense, but this language already assumes there is a real table of which we see the appearances.<sup>2</sup>

Russell believes there is difficulty in this assumption—the assumption that there is a real table of which we see many appearances. What difficulty?

As we walk round the table, its aspect changes, but it is thought impossible to maintain either that the table changes, or that its various aspects can all “really” exist in the same place (*op cit.* p 93)

If one assumes there is one table which we walk around, one *should* say that the table changes or that all the shapes it appears to have really exist in that place at once. But just why should we say either kind of thing? The difficulty, however it is described, concerns how appearances are related to the things which (we commonly say) they are appearances of.

H. A. Prichard introduces two other kinds of examples in one of his lectures, that of a mirror illusion (where an object really to the right of another appears to be to the left) and that of the

<sup>1</sup> This paper was begun while the author held a fellowship from the American Association of University Women

<sup>2</sup> Bertrand Russell, *Our Knowledge of the External World*, London, 1926, p. 84.

moon's appearing flat Of seeing reflections in the mirror, he says :

No body, if seen, can present the appearance which a body similar but reversed as regards right and left can present, nor as regards its relations to other bodies can it present an appearance similar to that which a similar body differently related to these bodies would present. A body, if it be really seen and seen along with other bodies, can only present to us just that appearance which its relations to the other bodies really require (*op cit* p. 53)

Of the second kind of example he says -

. . . This raises the question which the (former) case of an illusion raised, viz. how if we see a body it can thus look other than that it is and if we press this question home to ourselves we can only answer that it cannot (*op. cit.* p 54)

Prichard is emphatic that a thing ought to appear just as it is. With Russell, he finds something very wrong, even contradictory, in the idea of a thing having different appearances.

An altogether different summary of the problem of illusions is given by F. H. Bradley. He writes .

. Nothing is actually removed from existence by being labelled "appearance". What appears is there, and must be dealt with, but materialism has no rational way of dealing with appearance. Appearance must belong, and yet cannot belong, to the extended. It neither is able to fall somewhere apart, since there is no other real place (*Appearance and Reality*, p 12).

These philosophers express a common protest against the notion that a material thing can appear some way or other. The notion of "material thing" or "body" seems to them utterly discordant with the notion of "appearance", this is the most fundamental theme one finds in all uses of the argument. In ordinary and everyday speech we think nothing of combining the two concepts. It seems to us, indeed, that the queerest thing is to suggest that a thing *could not* appear in different ways. When one walked around a table, how odd if it should always appear the same! As one handled a penny, how queer if it should not appear elliptical as we turned it! When asked to reflect we think that a physical thing ought to, really *has* to, appear in different ways to us. It could not otherwise *be* such a thing. And a mirror which did not reverse things left and right would be substance for a dream.

Because both sides of the issue seem in their ways obvious, there is reason to make a detailed explication how the argument from illusion calls attention to a philosophical problem. It has a



logic against the right background ; it has force against a view which is very wrongly called either " naive " or " realism ".

## II

Let us characterize some of the cases which are fitted for the argument from illusion. In Russell's example, one walks around a table looking as one walks. If we had a rectangular table we would see that its appearance does change in a certain way as we look first from one side, then view it from a corner, then from the end. As we change our position we could sketch how the top of the table seemed to us, making " perspective " drawings. First it would look roughly trapezoidal and we should draw the top of the table in that shape. Then from the corner we would draw a lopsided diamond shape, from the end another and different trapezoid. We draw these shapes (and not a simple rectangle repeated three times) because we are trying to show how the table looks from different positions. Therefore let us say, from different positions the table does appear in these different ways. This is only one way of drawing the table. Another way is to draw that shape you would want a carpenter to follow in making a table like this one.

We draw a shape which is not the shape *of* the table. It is a shape we see ; we draw what we see. Many of us draw figures which we recognize to be similar, and we say they are good representations of how the table looks from here, from there and from there. Let us say simply they are *shapes which we see*. Nevertheless these shapes do not belong to anything before us.

In Prichard's second case, the moon appears to be a round disc in the sky. Here too someone could paint what he sees and not show the shape as a ball. He draws a circular disc. This is how the moon appears to him. He sees that shape, yet it is not the shape of what he sees.

In Prichard's first case we see objects in a mirror. This is a little different from the two examples above, for here we are in a certain sense not seeing the objects but their reflections, images. Nevertheless it is like the other cases because the shapes, the colours we see are not the shapes and colours of the mirror. The shapes, one can say, are the shapes *of* the coloured patches in the reflection, the colours are the colours *of* the figures in the reflection. But they are neither of them qualities of anything but one another. There is nothing besides the qualities conjoined. There is nothing else to which those qualities we see in the mirror strictly belong.

The argument from illusion is inspired by an instance in which with our eyes open and directed to some place, we perceive colour, figure, etc., not belonging to anything in that place. One wants to say, Do we really see them then? We do not "really see" them if this means they belong to an object where we look. But where we say, "The table appears diamond-shaped from here" and we draw a diamond shape as the shape that we see from here, then it is quibbling to deny that in some sense we are seeing *a diamond shape*. Where we say, "The house looks grey in the shadow" we can say instead that we see *the grey colour* it takes on, being shadowed. We see a stick partly immersed in water and say, "See how queer it looks—as if it were cut then glued together badly"; why not add, "See the queer shape?"

The argument from illusion raises this issue. Can qualities exist without belonging to anything? Can there be qualities which are suspended, not being possessed by anything? It is a problem for a philosophy which requires qualities to be qualities of objects. Under this assumption about the dependence of qualities on objects, it really is hard to say what is it for an object to appear in a certain way. How can the grey colour of a house in shadow be understood?

Suppose one says. Appearing to have a grey colour in the shade is itself a quality of the house. The grey colour depends upon the house being white, then, and is conditional upon the house being shaded. One would have to accept qualities which are thus dependent and conditional. And what could one say of the many appearances of a table? If a number of people stood around a table it would have many different appearances to the many people. It would have contrary appearances at one time. Surely these appearances cannot all belong to the table as qualities. The reason why there is a problem about appearances is just that they do not fit the mould of qualities; they are too temporary, too conditional and too profuse.

Then suppose someone says: Appearing is a relation which, like the relation of possessing or having, holds between things and qualities. The relation between the house across the street and the quality white is that of possession; the relation between the same house and the quality grey is that of appearing. This leaves the dilemma where it was. How can qualities exist without being qualities of something? The "relation" of appearing does not make appearances into qualities of anything; and how can qualities which enter into this relation exist other than as qualities of a thing?

It seems most sensible to argue that qualities may exist without belonging to things. The way we speak of qualities in ordinary life supports this and only a certain view about objects and qualities makes it seem repugnant. For two centuries philosophers have been led, with the help of the argument from illusion, to "construct" or "reduce" the world in a way which would avoid the problem of self-subsistent qualities. The solution I propose is much simpler, and gives no support to such a device.

### III

In the case of each of the examples which serve to present the argument from illusion, one perceives a certain shape or colour or combination of these even though it is false that anything one perceives has those qualities. If qualities must belong to things, then as Russell says we should say that the table changes shape or has many shapes at once.

On the one side the argument raises the question about self-subsistent qualities. On the other it makes us ask, How can we perceive objects except by perceiving their qualities? Like Berkeley, we hesitate to say we can see a thing barely, naked of the qualities it has. For what would this be? Yet when we are presented with a mirror image, not seeing any qualities of the mirror, this seems to be what happens. In a strict sense we see the mirror but the qualities we perceive do none of them belong to it.

Two examples will make these points clearer.

Suppose you enter a room which is fairly dark. Imagine there is a light coming through a window, and some areas in the room are light and others dark. You can make out a dark patch, darker than the rest of the wall, on one side of the room. You cannot see a shape or colour or even whether there is one object or a heap of them. A companion says to you, "That thing you see on your right (pointing to the spot) is a card-sorter". You know what you were seeing. Yet it would be absurd to say that since you saw the machine you *must* have seen some of its visible qualities. Seeing the object does not require this. You might say you saw a shadowy spot or, using Berkeley's phrase, an "unknown somewhat". But the important thing is that here you can still say quite naturally, "What I *saw* on my right was a card-sorter". It is not contradictory to see something without seeing *any* of its qualities. As it does not follow from the fact that someone sees a red object that he must have seen red, or from his seeing a rectangular object that he saw a rectangular

shape, or from his seeing an object with many knobs that he saw many knobby forms, so it does not follow from the fact that he saw a red, rectangular, knobby object that he saw any of these qualities

A number of examples commonly found in philosophical discussions about illusions are related to this one. It is often suggested that, when we see a cube, we should be able to see its six sides because it has six sides, and things such as sides are visible. And it is sometimes thought problematic that we cannot see the inside and the outside of a house at once, even though both of these are visible aspects of the house. Although we identify a cube as a cube by turning it around to see if it has six sides, we do not see it *by means* of seeing that number of sides which characterize it. We do not in general perceive objects *by means of perceiving their qualities*. This begins to show us how objects and perceivable qualities are related.

Now consider another kind of example. Imagine you are sitting in a room which has uniformly white walls and ceiling. You can see that easily. There are no splotches or variations in the paint colour from one part to another, from the walls to the ceiling. Yet where the sunlight strikes one wall there is a yellowish luminous colour, and this patch of yellow has a definite boundary against the unlighted part which is greyish. You could take a pencil and mark the boundary if you wanted, you could point to it and describe it ("It is like the outline of a human face"). Back where the walls form a corner, you can see the colour become increasingly grey into the furthest point of the corner. As the afternoon progresses this grey colour deepens. The yellow spot and its boundary move; it becomes less yellow; it grows smaller and finally disappears altogether. Now there are only different shades and qualities of grey.

Imagine you have a companion to whom you remark, "What a yellow colour the walls take on there, and there how grey they appear". He replies that he only sees the whiteness of the walls. You point to the yellow patch, marking its boundary with your finger. You point to the corner and ask if he does not see how *that* colour is different from the colour the ceiling takes on and from the colour of the yellow patch. He says he sees only white walls, uniformly white, unrelieved by grey or yellow or any other colour.

Surely you can say that he does see the yellow and the grey. If he is not colour blind and he looks where you point he *must* see it. He is only not attending to it as you want him to. But then, if he sees yellow and grey, is he mistaken in saying that he sees

uniform whiteness? Ordinarily you could say, "Of course, I know you see the white; it's the grey the walls take on I want you to notice". We should say, without thinking too much of philosophy, *he sees white, and there where he sees white he also sees yellow or grey.*

It is tempting to pronounce: No one can see white all over and yellow all over any more than a thing can be white all over and yellow all over. Like saying, Only one chessman can occupy a square. But one does not need to stop with this.

Is the grey colour really there? Anyone can see it; it is not in this respect subjective. Qualities which are subjective are, for example, those yellow appearances associated with having jaundice, or the reddish colours we see when first we come indoors out of the sunlight. This case is entirely different. The grey colour does not depend upon one's eyes in any way that the colour white does not, nor upon the state of one's mind. It depends upon the way the room is lighted. We can say it depends upon the position of the sun, then, and not upon a can of paint only. But surely this does not make it subjective.

What is the difference between the grey quality and the white one? Is it possible that the grey is a quality which merely *cannot* belong to objects? What would this mean? We can look at the grey through a frame, say made with our hands, and imagine that we are looking in clear light at a grey object. We can describe it. We can find an object which, in a certain position at a certain time of day, takes on exactly the same colour. We say, "They look exactly alike in colour" or "The colours are now just the same". We can compare the grey colour with other colours as easily as we can compare the white with others, only we do not do it in the same way.

The grey quality has some features different than the white besides the basic one that it is not painted on. It does not just have deficiencies compared to the real colour.

Several suggestions have been given about illusions which I think are wrong or at least unhelpful. One suggestion is this: The only colours which exist are colours of things. When one looks at white walls and sees them, there is only one colour he *can* see and that is white. He may think he is seeing another colour, but if he does he is mistaken. Against this view one wants to say with Bradley, "But saying the grey doesn't exist doesn't get rid of the grey". It is not like finding out one made a mistake, for the grey is still there when one knows the walls are white. By our eyes we perceive among other qualities, colours . . . grey, white, red, etc. One cannot say instead, "By our eyes we

perceive colours and something *like* colours which are not colours at all". For then one must be ready to say what is like a colour but a colour. One might say: "We perceive qualities which do exist and qualities which don't" But this is very queer. It would be more natural to say that we see qualities of things and qualities which are not of anything, two kinds of qualities, both of which rightly belong under the heading "What We Perceive".

The second way of dealing with this is to say that the grey and the white colours are both equally real. Neither of them "belongs" to the wall any more than the other, although the one may be visible more often than the other. I do not see how this solution would be made to fit my example, which requires us to account for seeing a white colour and seeing a grey colour in the same place at one time.

A third way of explaining my example is to say that there are two senses of "see" involved, and that in one sense you see the grey colour and in another sense you see the white colour. This solution respects the description I have given. But it does not tell us *what* we see in the sense in which we "see the grey" nor how it can be there and belong to nothing.

#### IV

The argument from illusion requires one to consider the nature of qualities which belong to things and qualities which only appear. I propose that there are two kinds of notions of qualities—two notions of blue, of warm, of most perceivable qualities—and that sometimes the words "blue", "warm", etc., are used to mean appearances and sometimes to mean qualities possessed by things.

One way to show there are two notions of qualities and not one with different aspects is by describing for each a language game which reflects the way that notion works in our language. If it is possible to describe two independent games, neither of them being derived from or dependent on the other, then we can view the notions as distinct and separable.

First let us try to form a game for apparent qualities, qualities which do not belong to things. In this game we want to take account of the various shapes one sees walking around a table, of the elliptical appearances of a penny, of the yellow and grey of the walls in my example. Besides these we should remember more common examples: the blueness of the air above us, the blue and green colours of ocean water, the stripes of a rainbow.

Suppose we were to teach a person the following game: We provide a screen which we can illuminate with various colours. The edges of the screen are concealed so that someone seeing it from the front can only see a part of its area. We can illuminate the screen with many different colours, or we can blacken it, or we can exhibit it as white. What we want to teach someone to do is say what colour he sees when we switch on a certain coloured light.

In this game we teach someone to answer this question: "What colour do you see there just now?" We point to the screen as we ask; he learns to respond saying, "Now I see blue there", "Now I see yellow". If he turns his back on the screen and then answers we tell him he must look again before he answers. This is the first part of the game.

After a student has learned to identify colours in this game, we introduce other situations. We take him out of doors and ask him as we point, "What colour do you see when you look at the sky now?" He answers that he sees blue. We ask him what colour he sees when he looks at the sea now, and he says things like, "It is dark blue-coloured at the horizon, but more greenish nearby". We ask him what colour he sees when he looks at a certain house and he responds, "Grey there, but white farther along the wall". When he can do these things I should want to say that he understands the notion of this game.

In this game colour words occur in certain kinds of expressions. "When I look there now I see, *e.g.* a lavender colour." "Now when I look at the house I see white". I will call this notion of colour the *here-now* notion. It does not depend upon the idea that colours belong to things. However, it does depend upon one's being able to *point to* things or know otherwise how to refer to them. A student of this game would not have got all that is in the language of *here-now* qualities if he could not show us *what* he is looking at when he sees a certain colour. The notion of *here-now* qualities presumes acquaintance with the world of things which cannot therefore be "constructed" from it.

It is not difficult to prove that the *here-now* conception of qualities is independent of the other conception. Suppose we ask the subject in this first game, "What is the real colour of the sky?" He looks up and replies, "Blue". If we teach him that air does not have colour, and therefore the sky has no colour, he will still say, "I see blue there". We have to teach him that "seeing blue there" is different from "seeing *something* blue there". We need to add to the *vocabulary* of the game. Suppose we ask him what colour a thing is when it is far away and hard to see. If he replies at once we say, "No, you can't answer just

by standing here and looking. You must go closer." We need to add a *procedure*, or several procedures to the game. Nothing in the game he has learned would suggest these two additions. It would be correct to say that they do not belong in *this* game. In a certain sense they "contradict" the rules.

We could revise the game above so that instead of colours illuminating the screen, shapes were projected. We could teach a subject to describe figures and shapes very precisely. Then, taking him out of this setting, we would encourage him to describe the shapes he sees as he moves around objects. shapes he sees in a mirror and so on. We should then have an analogue to the game for colours.

Now consider qualities which are attributed to things. Imagine the following game. We present someone with different coloured papers and, holding them in a good light where he can see them we say, "This one is red", "This one is blue" and so on through all the papers. We teach him to identify each of the colours, to point to each paper in turn and say what colour it is. Suppose then he carries the papers around with him. When he goes into a darkened room he learns to turn on the light before going through the pack and identifying the colours. If the lighting in a certain place is queer we teach him to wait until the light is clearer. Suppose he wants to know which colour each paper is without always having good light? He takes them into good light and writes the name of each colour right on the paper. In this game he can read off the names of colours when he could not tell the colours just by looking.

In this game we prove that the sky is not blue by proving that air is colourless. We do the same for water. Such proofs are in the game.

As part of this game, we introduce guessing. In a darkened space we make guesses about the colours of things. Then they are taken out into the light to see which guesses were right and which wrong.

The kinds of expressions in which colour words appear in this game are such as "I would guess that is blue", "I can't tell whether it's blue", "I will find out if it's blue", "It's not blue as I thought", "I remember it is blue". "It is blue". Many more kinds of expressions could be added.

Suppose we were to introduce into this game the question of the other game "What colour do you see there, now?" If we are pointing to a white house in shadow, we do not want to hear: "That's a white house. I can see its white colour." Yet what other kind of answer can be given in this game? One *should* be



baffled. The here-now notions of colour have to be introduced independently; they do not arise naturally or logically out of the predicative notion.

One wants to say: Well, surely one can say how the house *appears* if one can say what colour it is. But the game does not include that; that is another matter. In this game colours cannot just hang before us belonging to nothing. One may believe at some time that a rainbow is a coloured *thing* in the sky. This too belongs in the game; in this game it is possible to make mistakes. But one cannot say, "The colours *are just there*".

Each of the two games I described reflects part of our language concerning perceivable qualities. Our language contains both sets of notions; they are made to work side by side, neither requiring the other.

## V

If we can separate out of our ordinary talk about perceivable qualities two kinds of quality-concepts, where does this leave their likenesses? If there are two notions of red, for instance, how can one account for the fact that a red appearance and a red thing are both *red*? In the one sense "red" does not mean something more bluish or more pale, in one sense it does not mean something with less range than the other, nor with less intensity. The sense of "quality" may differ, but "red" is universal.

This objection urges us to say that there is only one sense of quality words because there is no "qualitative difference" between a red appearance and a red surface. I have argued that there are two notions of quality between which it would not make sense to speak of qualitative differences. The difference between them is in the concepts or in the two language games.

Consider the matter of comparing qualities. Suppose, having seen the ocean at dusk, you try to describe it by holding up a shimmering bluish fabric. Held in the light a certain way it looks very much as the ocean did, you say. Now you are comparing here-now qualities, and you say that one resembles the other. There is no problem in this.

Imagine another kind of case. You are trying to show someone just the colour of paint you have selected. You say, "Here, it's the colour of this bowl". We can look at the bowl, examine its appearance under yellow light, in shadow, under many conditions, to see how the blue of the paint you selected will appear under similar conditions. This is comparison of predicated colours. There is no problem in this.

Imagine still a third case. You speak of the evening sky, how it was streaked with orange at sundown. "This orange", you say pointing to a piece of coloured paper, "And just these colours of blue and gold" pointing to other pieces of paper. Are you now saying that the colours of the papers are the same as the colours which *appeared* in the sky? If we took the papers into another kind of light, would not they be different colours from colours in the sky? Then what is being compared here are here-now qualities again. We do not have a here-now quality being compared with a predicated one.

How can one speak of two kinds of qualities being qualitatively the same or different? Prichard, Russell, Broad, and many others as well have thought that it was important to point out that one can imagine situations in which (to use Prichard's words) "a certain colour or a certain complication of colours would be indistinguishable, so far as seeing goes, from a certain coloured body if there were such a body and we saw it from a certain point of view" (*op. cit.* p. 62). That is to say, we can imagine deceiving someone with a projection on a screen, or with a mirror, so that he thought an object was before him which was not there. He was deceived because we arranged things so he should see an appearance which was *exactly like* what he would see if those objects were now before him. On my view it would be described as a case in which the here-now qualities he sees are just like the here-now qualities he would see if the objects he expected (from that place, in that light) really were before him. The mirror is not *like* a view of the garden; a projection on a screen is not *like* a city street. But from a certain point of view, as Prichard says, one would see the same "complication of colours".

Are the here-now senses of quality words fundamental to the predicated senses? From an affirmative answer to this philosophers conclude that when we "see" predicated qualities, the qualities of things, we are really making an inference of a certain unique kind from here-now qualities. This kind of view was held by Russell, by Prichard, by Broad and many others. My argument shows that the language game of predicated qualities does *not* require here-now notions. If that game reflects a part of ordinary talk about qualities, then it certainly does follow that in the kind of world there is this game is just as "fundamental" as the other game. Neither rests in any way upon the other. I think this is the correct view of the matter.

Given the kind of world we have the two language games are independent and equally fundamental. But we can ask, Is it possible to conceive a world in which the here-now notions would

still apply but the notions of predicated qualities would not? Then can one imagine the converse? I propose in answer to the first question that we *can* imagine such a world ; the answer to the converse question is we *cannot*. The proof of this shows that here-now notions of qualities are in a certain sense *more primitive* than those of predicated qualities.

The proof I have in mind is this.

The qualities things really have are in many cases not very changeable. If I buy a red hat, looking at it carefully in daylight, it will be red when I get home. If I open the box at home and find a blue hat inside it is the wrong hat. It cannot be the same one. The colour one paints on the walls of a room will be the colour they have after a week, six months, a year. It takes a long time for paint to fade or felt to discolour. If you saw a hat or the walls of a room under good light last week and they were not re-coloured (repainted or dyed) in the meantime, then you can assert *without looking* what colour these things are *now*. The colours of things persist. Some are constant for longer times, some for shorter. But for anything which *really has* colour there is a time interval during which it would be unquestionably true to say that that thing is the colour which you last saw it to be, barring a change especially made.

The persistence of predicated qualities is one requirement for there *being* predicated qualities. Imagine a world in which qualities change so unpredictably that there is no interval of time during which it would be unquestionably true to say that a thing *had now* the quality which you last perceived it to have. Imagine that the hat one buys at the store might be a different colour when it arrives home, that it changes colour while resting in the closet, that it changes again when one wears it. Its colour changes at varying intervals ; one cannot say, " It is going to be blue today " or even " It will be blue five minutes from now ". Imagine this is true of the paint one buys at the store - it changes by the time one opens the can, changes again after it is applied, and continues to change from time to time. In such a world would we ask at a store for a red hat? Or for green paint?

Under these circumstances we would hesitate to think we could " find out " what colour a thing had by providing better light. We have not that assurance of an interval of time during which its colour will persist. We take a darkened object into clear light and say, " It's blue now ", but not " It *was* blue ". Although there is still a contrast between not seeing what colour a thing is and seeing its colour, there is no clear sense to " finding out " the colour of a thing which one does not now see well.

Without looking at an object any reference to its colour will be unhelpful in identifying it "Bring me my brown purse", "Wear the black coat", "Where's the red pencil?" are equivalent to the same sentences without colour words included except when one is looking at these things. Thus being so, one may say that the predicated notions of colour have been lost. They have been replaced by a weaker set of notions which are only reminiscent of predicative ones.

I have discussed the possibility of a world in which predicative notions of colours do not fully apply. One can imagine worlds in which each other kind of quality—shapes, sizes, smells, tastes—could not have predicative senses. However, if one imagined a world in which *none* of the various kinds of qualities could be predicative, it is likely we should have lost most, if not all, of our everyday conception of "objects to which qualities may belong". In a logical way it may be that objects depend upon there being some predicative qualities, as in a metaphysical way the dependence is reversed.

It remains to ask whether in the world I described the here-now notions might remain as they were. And I think it is easy to see that they might. It must then sometimes be possible, of course, to point to an object and say, "When you look there you see red" and for another person to ask, "You mean there?" and for one to reply, "Yes, there", and so on, and all this while the colour persists. The world I described, although unpredictable, would allow this *sometimes*, and that is all that is needed. The predicative notions might lose their application to the world, then, while the here-now notions were unaltered.

Now consider, could we imagine a converse situation—a world in which the predicative notions apply but the here-now notions do not? This would mean that objects could not have different appearances from different places, that there was nothing which like air or water or mirrors seemed to have a colour it did not have, that things had the same appearance under a blue light as in daylight, and so on. And I confess this seems to me beyond the power of imagination. Given the notion of "objects having perceivable qualities" I believe it is *necessarily true* that objects offer different appearances from different places and under diverse conditions. This is to say that one *could not* have a language of predicative qualities where a language of here-now qualities was impossible.

This assertion is not to be confused with the assertion that the *notions* of predicated qualities depend upon those of here-now qualities. That I denied. But the *conditions*—the state of the

world—required if one is to have a language of predicated qualities is such that it cannot exclude the *conditions* necessary for a language of here-now qualities. In *this* sense one can say that the here-now qualities are more primitive than predicative ones.

I think it is obvious that instances of here-now qualities would serve as instances of what many philosophers call "sense-data", and the proposition that sense-data are more primitive than predicative qualities is something these philosophers often assert or imply. The characterization of here-now qualities is of course not that of sense-data. And the sense in which here-now qualities are primitive is considerably weaker than what sense-datum philosophers have maintained.

The foregoing arguments bear on a number of propositions commonly made or implied in discussions of illusions.

(1) When we see an object (or think we see one) we see one shape and one configuration of colours. For example, if we see a penny from an angle we see an elliptical shape and no other. Against this proposition I maintained that it ordinarily makes good sense to say one sees an elliptical appearance *and* the round shape of the penny both at one time.

(2) When we make mistakes about the qualities of things, we must mistake the apparent colour for the real colour, and both colours must be of one kind or we could not mistake them. I have said that we do not mistake one kind of quality for the other. The assertion that one sees a red colour in the here-now sense is independent of the assertion that one sees a red thing; one may see both or only one or neither. To show that we did mistake one kind of quality for another it would be necessary to prove, I think, that we cannot see both kinds of quality at once (*i.e.* to assert proposition (1) above) and that is false.

(3) The here-now sense of quality words is fundamental to the predicative sense and we need to infer or construct judgements about the qualities of objects from the appearances we perceive. This is false if the language game of predicative quality notions is consistent and independent of the other kind of game. For there was no inference or construction required there. There was occasion for doubting, for guessing and checking to be sure; but no inferring.

The notion of "objects" which as we say are physical and perceivable is a very difficult one to describe. I have only roughly sketched how it is related to perception and predicative qualities. One purpose of this discussion was to open the way for dealing with that subject more clearly and without that

prejudice which has been associated with the argument from illusion <sup>1</sup>

<sup>1</sup> In a brief article "The Argument from Illusion", in *Contemporary British Philosophy* (3rd ser.). H. H. Price enunciates a view rather similar to the one presented here. Speaking of "illusions" of perspective he says: "The belief that there is something illusory about perspectival size . . . that there is even a kind of contradiction in it . . . these beliefs have arisen from confusing the two different sorts of size, field of view size on the one hand and physical size on the other" (p. 395). He does not explain what is involved in there being "two sorts of size"; it may be he has in mind something like two notions or concepts of "size", in which case his account of the argument from illusion would agree in some points with mine (the case of "size" seems me be more questionable, however, than that of shape or that of colour).

### III.—APPROVALS, REASONS AND MORAL ARGUMENT

BY GEORGE C. KERNER

#### I

How may an utterance of the form 'I approve of  $x$ ' be defended by arguments? Occasionally we might try to show that the speaker was sincere, that he really had a favourable opinion of  $x$ . This is appropriate in those, comparatively rare, cases when the form of words 'I approve of  $x$ ' serves as a description of what the speaker inwardly feels. In those cases the supporting argument may also consist in showing that the speaker really knew his own mind and was not misled concerning his own sentiments. More frequently and more naturally, however, when the words 'I approve of  $x$ ' are spoken they serve as an endorsement of  $x$ ; and the concern with the state of the speaker's mind or with his self-knowledge is a concern with what is only indirectly implied and not with the core of what is directly said. If the words 'I approve of  $x$ ' function as an endorsement, as they typically do, they do not describe the mental disposition of the speaker and hence cannot be supported by any statement or collection of statements which does so.

What, then, is the nature of the reasons and arguments by which a linguistic act of the form 'I approve of  $x$ ' may be defended? Suppose I say, issue the endorsement, 'I approve of Jones' and am challenged. What form of defence may I give to my words? Had I said 'Jones is 5 feet 10 inches' or 'I am in love with Jones', the appropriate kind of thing to do would have been to try to show that what I said was true, that is, to give evidence for Jones's being 5 feet 10 inches or for the existence of my affection for him. In our case, however, that would not do: if 'I approve of Jones' functions as an endorsement, it is not in any straightforward sense true or false. Nevertheless, my words 'I approve of  $x$ ' *imply* certain matters which are capable of being true and false—namely, certain statements about (a) Jones, the object approved, and (b) myself, the speaker. In defending my endorsement, 'I approve of  $x$ ', I am thus confronted with a double task: I must first show that the object of my approval possesses certain characteristics, that is, that it meets certain standards; second, I must show that certain things hold true of myself, that is, that I possess the appropriate competence and have exercised a due amount of care and acumen.

Whenever we approve of somebody, some object, some practice, we always approve of it as something of a certain kind and as playing a part in a wider context or serving some function or end. I approve of Jones, as the captain, for the morale of the team ; of steel chairs, as furniture, for the new house ; of strawberries, as dessert, for dinner ; of baseball, as a sport, for children. Such specifications are always present, if not explicitly stated, then presupposed and supplied by the context. Another point about approval is that we typically approve in a certain capacity, role or standing. I approve of Jones (as the captain for the morale of the team) as the coach, of steel chairs (as furniture for the new house) as the owner, of strawberries (as dessert for dinner) as the host ; of baseball (as a sport for children) as a father or as a teacher. To these two points correspond the two lines of defence incumbent on me when my utterance of the words ' I approve of x ' is challenged.

Since to approve of x is to view x against the background of a wider context of functions, purposes and ends, it makes reference to standards. Suppose I serve on the Admissions Committee and say, ' I approve of Jones as a new student '. If my utterance is challenged, I may justify it by showing that Jones meets the requirements for admission of new students, that is, present an argument of the following form :

If the applicant possesses qualifications A, B and C, he may be admitted.

Jones does possess qualifications A, B and C.

∴ Jones may be admitted.

But this might not be the end of it ; the case may be more complex. Often the standards and rules that guide the work of committees and juries go only so far . they only amount to laying down the necessary, but not always the sufficient, conditions for an action to be taken or a verdict to be delivered . It may be that everyone is agreed that Jones does have qualifications A, B and C—those sufficient in normal cases—but still the question remains whether or not Jones's application should be approved. One may claim that the rule ' If the applicant possesses qualifications A, B and C, he may be admitted ' is not enough to decide *Jones's* case favourably. And suppose that, no matter what qualifications of Jones's and what rules or precedents I cite, my fellow members of the Admissions Committee insist that I further justify my approval. I am expected to do better, but how can I do better? Since I have tried all the recognized principles and rules, no clear-cut " proof " is any longer open to me.

There are, nevertheless, several things that I might still try.



I might say (1) 'I know boys like Jones and they invariably do well'—claim to have special experience and insight relevant to the case, (2) 'I have known Jones since he was a child'—claim to have special familiarity with the case being judged, (3) 'I have served on this committee for fifteen years'—claim to have ample experience in my job, or (4) 'I had a talk with his headmaster the other day and he firmly believes that Jones would do well'—refer to the testimony of someone else. These remarks are by no means absolute clinchers, but they all carry some weight and I can always think of other and better ones. What is important to notice is that they all are considerations relating directly to *me* and only indirectly to Jones. In approving of something we are, characteristically, doing a certain job, playing a certain role, serving in a certain capacity. Therefore, once my impersonal "proofs" have failed, I can further justify my approvals by showing that I am, in the given case as well as in general, fully qualified to do that job, to play that role, to serve in that capacity.

We can now bring out another important point. Suppose that I finally did succeed in convincing my fellow committee members through my second line of defence. My success would then have created a precedent, or even a new rule. Some points about the character, history and achievements of Jones (X, Y, Z) which until now had gone unnoticed might hereafter become *qualifications* for admission. Jones would have become a test case for the principles guiding the work of our committee: what was at issue was no longer just whether or not Jones was to be admitted, but also what should be the requirements for admitting new students in general. And my endorsement, 'I approve of Jones', would have had, for all intents and purposes, the force of 'I hereby subscribe to or enunciate the rule, "If the applicant possesses qualifications X, Y and Z, he may be admitted"':

We may now sum up the situation concerning the defence of utterances of the form 'I approve of x'. To say, to issue the endorsement, 'I approve of x' is not to describe my attitudes or feelings; nor is it to describe x. As an endorsement it contrasts with such utterances as 'I am in love with x' or 'X is 5 feet 10 inches' and its justification does not, therefore, in any straightforward sense, concern its truth or falsity. To say 'I approve of x' is like saying 'I endorse or certify x as a qualified so-and-so'. If I am called upon to defend that linguistic act I may, therefore, have to show (1) that x meets certain requirements, that is, deliver an evaluative "proof". But since to say 'I approve of x' is not just to state that x meets such requirements, this first line of argument might not be enough. I might have to go on

and show (2) that I myself have certain qualifications, that I possess and have made use of abilities which make me competent to issue such an endorsement or certification

It may be objected that this procedure could never *really* justify an approval or show that it was really right or well advised.

If we take this objection to mean that it can never be really shown by this method that in a given case the approval was justified, right, or well advised, it has already been amply answered. If after having said 'I approve of admitting Jones' I show that Jones fulfils requirements A, B and C—those sufficient in normal cases—and/or that I possess the experience and competence for doing my job well and that in the given case I have exercised the proper amount of care and acumen, have not been blinded by personal prejudice, etc., what further justification can be asked for? It is always possible that, in a sense, I failed nevertheless, it might turn out that Jones becomes a menace and a disgrace to the college: fails all his courses and elopes with the President's daughter. But what more could I do? I cannot make myself infallible. If I say 'I approve of admitting Jones' I imply that Jones is worthy of being admitted. If after I have shown that Jones is gifted and industrious, possesses good character, has a good scholastic record, is an outstanding athlete, etc, etc, I am still told, 'That does not prove it', I would no longer know what to say. Or, supposing the case is in some way unorthodox and does not come under the established rules, if after I have shown that I have studied Jones's record carefully, had a long talk with him, his father and his principal and, further, that I have done my job for many years with admirable success, etc, etc, I am still told 'You are nevertheless not competent to judge this case', I would again lapse into puzzled silence. If my endorsement is challenged, that challenge is meaningful only if there is some indication of a more or less definite defect in my case and, hence, of how the challenge could be met. If there is no such indication, then rational justification of any kind has come to an end.

On the other hand, the above criticism may raise the question whether or not there is a way in which practices such as judging college applicants by boards, of which endorsements like 'I approve of x' are parts, can themselves be justified. Now, in one sense, it may be said, there clearly is not. There is no way *within* a practice. In our case, what can be discussed within the practice is the justice or advisability of individual cases of approval and not the justice or advisability of the whole practice of judging college applicants by a committee. The fact that there is such

a barrier comes out when, in discussing the Jones case, my recalcitrant colleagues make me blurt out 'I am a member of the committee, am I not?' The exchange between me and my fellows has then, in one sense, come to an end. I have invoked the very existence of the Admissions Committee as an institution. But in another sense we could still go on. Suppose that in answer to my last piece of rhetoric someone remarks 'Yes, but these things (admissions) are better judged by lot anyhow'. He has now moved the discussion to a different level—to a level, perhaps, where there would be a way in which the justice of the practice of judging college applicants by boards could be decided had the occasion been the meeting of the whole governing body of the college rather than that of the Admissions Committee. Though the justice of a practice cannot be discussed within that practice, this does not mean that there cannot be other practices in which it can.

The last paragraph contains an oversimplification. We have omitted the important point, mentioned before, that there are test cases. We may now elaborate on it. Practices like admitting new students through committees have a self-corrective character. Within such practices there are performances which shape the character of the practice itself. In discussing the nature of the justification of the endorsement 'I approve of  $x$ ' we said that often that justification takes the form of showing that the object approved fulfils the requirements laid down by certain accepted rules. In those cases 'I approve of  $x$ ' is justified just in case  $x$  satisfies the rules or requirements  $r_1, r_2, \dots, r_n$ . But we also saw that there are unorthodox cases where  $x$  does not come under any such existing rule. In such unorthodox cases the force of the words 'I approve of  $x$ ' goes beyond being an endorsement of just the object in question and amounts to subscribing to or enunciating a new rule or principle,  $r_{n+1}$ . We claimed further that a justification for the words 'I approve of  $x$ ' can also be given in these more consequential cases and that such justification takes the form of pointing out certain facts relating, at least directly, not to the object approved but to the speaker. It follows that if such justification is successful, then the whole practice of admitting applicants by the approval of the board has undergone a modification: a new rule,  $r_{n+1}$ , has been added to the old ones,  $r_1, r_2, \dots, r_n$ .

Now one might claim that if the second type of justification—justification in unorthodox or test cases—is to count as justification at all, we need another pre-existing set of rules,  $R_1, R_2, \dots, R_n$ , in order to decide which facts are to count as the relevant

ones when cited about the speaker, and that therefore the decision on test cases cannot really count as a modification of revision of the existing practice. But what would be this other set of rules? It seems that it would have to be a heterogeneous lot. When I am forced to take the second line of defence in justifying my endorsement 'I approve of x' what I have to do is to show (1) that I am well qualified for my job, and (2) that I was doing it well in the given case. Under (1) we might thus list such things as past experience and success, integrity, loyalty, etc. Under (2) we might mention familiarity with and care in studying the particular case, impartiality, etc. That one could do any better, be more rigorous, in specifying the relevant matters is doubtful. There simply is no set of rules that lays down the necessary and sufficient conditions for members of college admissions boards to do their job well. In defending my endorsement I just have to wait and see what sorts of challenge are in fact raised and build my defences accordingly.

But a further complication must be mentioned. In the case of official practices such as judging college applicants by boards there are, nevertheless, some rules and requirements which in a minimum sense do lay down both the necessary and the sufficient conditions for the adequate defence of utterances of the form 'I approve of x' in test cases. As we saw above, when driven into a corner or when encountering exceptional obscurantism, I might, in defence of my words, take the last resort and say 'I am a member of the Board, am I not?' With this move, as we mentioned above, the boundary of the whole practice of admitting new students by the Committee has been reached; any demand for a further justification would, in a sense, take us outside that practice I have invoked, the rules that define the whole practice we follow or make up the very constitution of our Committee, and I have pointed out that by virtue of these rules my words carry a certain ineluctable weight. If, at a meeting of the Admissions Committee, I say 'I approve of admitting Jones', but it turns out that I was not really a member of the Committee as my appointment had not yet come through, my words would not have that weight. My endorsement of Jones would be null and void.

In describing the conditions which, when fulfilled, *would* make the utterance of the words 'I approve of x' in an official context into a valid or *bona fide* endorsement, we are describing the conditions which, when fulfilled, would, in a minimum sense, also justify that endorsement. Besides uttering or writing the words 'I approve of Jones', there are other conditions and

circumstances which must be present before it can be said that there "really" was an approval at all. Perhaps it must be done on a certain form, or in red ink, or uttered when all, or the majority, of the members are present, or whatnot. And, of course, the speaker must be a regular member of the committee, duly appointed, etc. Assuming that all these matters are in order, then in one sense of the word 'justify', in the sense of 'validate' or 'legalize', all that the speaker has to do in order to justify his endorsement is to show that by virtue of the constitutive rules of a practice such an endorsement comes under his jurisdiction. In other words, he need but point out that he was *officially* in the position to "really" approve of *x*. And once he has done so, the matter is closed. There is no longer room for argument. authority has taken the place of reason.

In a sense, then, all I might have to do in order to justify or defend my endorsement of the form 'I approve of *x*' is simply to exhibit the fact that I possess the appropriate authority. But surely, only in a sense—in the sense in which such matters as judging college applicants are "merely" formal and official business. In so far as they are not, endorsements like 'I approve of *x*' have to be defended and justified, first by reference to accepted norms and standards, through evaluative "proofs" and, ultimately, by more ephemeral or at least more elusive and complex arguments concerning the qualities of the speaker himself, which derive their force not from a set of fixed rules but from the more general and unclassifiable fund of human concerns, ingenuity and *ad hoc* inventions.

## II

Much of contemporary moral philosophy is vitiated by the neglect of this last point. There has been an increasing emphasis on the non-descriptive character of moral language. Moral judgments are no longer looked upon as descriptions of either natural or non-natural facts but as prescriptions, commands, commendations, endorsements, etc. This, I think, is salutary. But with this insight new problems have arisen, the foremost among them being the problem of moral reasoning. If moral judgments are not descriptions of fact, they are not in any obvious sense true and false. What, then, is the nature of the arguments and reasons which can support them? Above I have offered a sketch of how reasons are given in support of one type of non-descriptive utterance. This sketch, I believe, would be helpful in the clarification of the nature of moral reasoning. But an attempt to apply it to

the whole problem of moral reasoning would take us well beyond the space of this paper. I shall merely indicate how the results of the above discussion point up the inadequacies of two theories of moral reasoning which have so far arisen in conjunction with the non-descriptivist thesis about moral language and enjoy a wide currency today. The first of the theories which I propose to consider briefly is propounded notably by Toulmin, the second notably by Stevenson and Hare. There are, of course, differences between Stevenson and Hare, but for our purposes these may be ignored. Since the writings of all these authors are well known, I shall keep the exposition of their views to a minimum.

While in the views of Stevenson and Hare it turns out that moral judgments and principles have in the end, nothing else behind them but arbitrary attitudes and decisions, Toulmin in his *Reason in Ethics* insists that for "fully developed" moral judgments there are truly "good reasons" which can be given in their support. In moral reasoning, he claims, a special kind of inference is used—namely, what he called "evaluative inference"—by which "we pass from factual premises to an ethical conclusion" (p. 38). Thus, for Toulmin, moral reasoning consists in delivering what I have called an "evaluative proof", in showing that the object in question fulfils certain requirements. And, he claims further, what these requirements are is ultimately summed up in the requirement of social harmony: an action or social practice is right if, and only if, it involves "the least conflict of interests attainable under the circumstances" (p. 224).

What is the status of this last claim? Toulmin believes that this claim is a logical one, that the principle of social harmony specifies the logic of moral discourse in a nutshell since, and this is his main argument, the promotion of social harmony is the very function and purpose of moral language (e.g. pp. 146 ff.) Now if this were so, if Toulmin's thesis were a logical one, there would be nothing that any individual could do in order to use moral language in any other way but for the promotion of social harmony. But clearly, to use moral language in that way involves choice and effort. It is quite possible that, contrary to all general practices, an individual will use moral language for promoting aims not shared by others. And I do not mean that he may use moral language propagandistically for the promotion of his own selfish and immoral aims. One can, logically, use moral language correctly and at the same time follow aims which conflict with those of one's fellow men. On the other hand one may, logically, misuse moral language in spite of the fact that one is trying to promote social harmony. It is conceivable that

a whole community, or even the better part of mankind, would lapse into a misunderstanding concerning moral language and begin to misuse it. And should this misuse be radical enough, then, in a sense, morality could be said to have disappeared from that community or from the face of the earth. But at the same time, people could still continue to pursue harmony in their purposes, interests and aims.

If Toulmin's thesis is not a logical one, how then should it be interpreted? There are two possibilities: his thesis is either a contingent empirical generalization about the aims and goals which people in fact have in mind or try to pursue when they use moral language, or it is a recommendation concerning moral standards. There is some evidence for the justice of either interpretation. Toulmin claims that what he says is not a "theory" at all but a description of the facts of usage (p. 144). And we must agree that socially accepted morality is always, or at least usually, determined by the principle of harmonization of interests. There is, however, nothing inevitable in this. There may be, and probably are and have been, societies in which the recognized moral duties are such that, to a certain extent, they undermine the harmony between the interests of its members. In fact the only society in which this could not happen to any degree would be an ideally utilitarian one. But surely, it would be odd to say, as Toulmin seems to, that only in such a society would people know how to use moral terms and arguments correctly from a logical point of view. If Toulmin's thesis is to be taken as an indirect way of making a sociological point, nothing could follow from it concerning the nature of validity in moral reasoning. It would seem, therefore, that the second alternative of interpreting Toulmin's doctrine is the more natural one. If we take the principle of harmony as a recommendation, it belongs more properly to moral philosophy. But this choice has its own difficulties. There are no good reasons given by Toulmin for adopting the principle of harmony as the supreme moral principle. Moore in his *Principia Ethica* wrote that since the term 'good' is indefinable, "nobody can foist upon us such an axiom as that 'Pleasure is the only good' or that 'The good is the desired' on the pretence that this is 'the very meaning of the word'". We can adapt Moore's words and turn them against Toulmin by saying that since not even the criteria of application of such words as 'good' and 'right' are necessarily identical with conduciveness to the harmony of interests, nobody can foist upon us such an axiom as that 'An action or social practice is morally right if, and only if, it contributes to the harmonization of

people's interests' on the pretence that this is 'the very function or purpose of moral language'.

Toulmin himself had to admit that "'This practice would involve the least conflict of interests attainable under the circumstances' does not *mean* the same as 'This would be the right practice'" (*op. cit.* p. 224). But if the principle of harmony does not sum up the whole meaning of moral judgments, there must be moral questions to which that principle does not provide the full answer. In fact, to deliver a proof in defence of a moral judgment, no matter what the principle in terms of which the proof is conducted, is never absolutely final, since which rules of inference may be legitimately used in conducting such a proof is itself a moral question. We saw in the first part of this paper that an evaluative proof is only part of the process by which an utterance of the form 'I approve of *x*' may be supported. Similarly, a moral proof—an argument attempting to show that a moral conclusion follows from a set of factual premises—is only a part of the total activity through which a moral claim is supported by reasons. In order to have a proof I must have rules of inference. Therefore, by delivering a proof I have committed myself to certain general principles. But I must be prepared to show, should the occasion require it, that I had the needed competence for subscribing to them. Furthermore, there are unorthodox cases which do not come under any pre-existing principles and where new principles are therefore needed. In order to be able to construct a proof, I might have to enunciate new principles. And, when called upon, I must then demonstrate that I am well qualified to do so.

Let us now turn to the second type of theory concerning moral reasoning which has come to be widely held and discussed today, that represented by Stevenson and Hare.

Stevenson realizes that the question of moral principles and standards cannot be settled once and for all on "logical" grounds. As he argues in his *Ethics and Language*, that question is itself a moral issue. His main point is that moral standards—or the "descriptive meaning" of moral terms (that goes beyond reference to a pro-attitude of the speaker)—are always specified by "persuasive definitions" (pp 207-8). For him, to say "'This is (morally) good'" has the meaning of "This has the qualities or relations *X, Y, Z . . .*" 'is to pass a normative moral judgment, *i.e.* in Stevenson's terms, to give vent to a pro-attitude or favourable feeling. In our analysis of approval we made a somewhat similar point. We claimed that the standards or rules in terms of which endorsements of the form 'I approve of *x*' are justified



in "normal" cases are themselves laid down by such linguistic performances in "test" cases. Approvals are typically parts of self-corrective practices.

Nevertheless, our conclusions differ significantly from those of Stevenson. According to our analysis, approvals that constitute precedents and have the force of laying down new rules are themselves open to rational argument. The test cases are not arbitrary, unarguable ventings of attitudes and emotions. They too can be reasoned about, although in their case the argument does not rest on a clearly circumscribed set of rules or principles. Now there is, perhaps, a minimum approval through which the only endorsement given is such that any one of us possesses the required competence for giving it by virtue of the sheer fact that we are human beings endowed with the ability to have emotions and attitudes. There may be utterances of the form 'I approve of x' where all that is implied is that the speaker happens to have a pro-attitude or favourable feeling of some sort toward x. To defend *such* an endorsement *would* be just to point out that fact. But surely, moral judgments are not utterances of such impressionistic character and the reasons which we may be required to give for them go beyond showing that the speaker, a human being in full possession of his emotional capacity, happens to have a certain attitude or feeling. The mere existence of an attitude or feeling does not silence moral criticism.

The same inadequacy is present in Hare. While for Stevenson the question of the descriptive meaning of moral terms, that is, the question of moral standards, is settled by arbitrary attitudes, for Hare it is settled by equally arbitrary decisions. Hare argues in *The Language of Morals* (p. 111) that, for example, the assertion 'This is a sweet strawberry' can serve as a reason for the assertion 'This is a good strawberry' only if we have the appropriate "major premise"—that is, the specification of sweetness as one of the standards of goodness in strawberries. And what such standards or, as Hare calls them, "principles of choosing" are, is for him a matter settled ultimately by a decision, a "decision of principle". Nowhere does Hare explain how such decisions of principle may themselves be supported by arguments or reasons. In fact, it becomes plain that in his theory they cannot be supported by reasons: decisions themselves create principles, that is, reasons. Contrary to this view, we emphasized that although it is true that decisions, decisions to issue an endorsement, are capable of creating new principles, they can do so only if they are successful in the face of rational criticism—that is, if the speaker can demonstrate that he possesses the required competence and

ability, that he is in the position to enunciate new principles. What constitutes competence in moral matters? A discussion of this question would no doubt involve one in taking sides with respect to normative moral issues. It is here, I suggest, in discussing the qualifications of the moral judge and legislator, as well as of the moral agent—of the moral person, if you like—rather than by laying down moral principles, that moral philosophy should make contact with normative ethics. Now these matters are not at all explicitly discussed by Hare, but there are passages in his book which suggest that he would have to say that what makes us into well qualified moral legislators is the sheer fact that we are human beings. According to him the circumstance that we all have to make decisions—that we all have to go on with the business of living—sums up our moral being. He says that moral principles both apply to and come into being through the decisions of men as men (p. 162). But such an answer is clearly inadequate. In connection with Stevenson, we pointed out that our human prerogative to have likes and dislikes does not silence moral criticism. We may now add that our human predicament of having to make decisions does not silence it either. Once I have shown that I have subscribed to or enunciated a moral principle as a man, as a being who constantly and unavoidably finds himself faced with the necessity of making decisions, it can still be asked of me, 'Yes, but was it right of you, were you in the position, to subscribe to or enunciate that principle?'

It seems that what both Hare and Stevenson have really done is to view morality as a kind of all-embracing institution, as a popularized Kantian Kingdom of Ends perhaps, made up of men "as men". And therefore to them moral reasoning appeared to be basically similar to what we have described in the first part of this paper as the third type of argument by which utterances of the form 'I approve of x' sometimes may be supported—the argument from authority or official status. When my moral judgments are challenged, then, in the end, all I can, and need, say is 'I am a human being, am I not?'—that is, invoke the authority and status which I possess as a member of humanity through the sheer fact of being a creature endowed with sentiments and confronted with the necessity of having to make decisions. But such a view would be clearly fallacious. Invocation of authority may be, as we saw, looked upon as a reason or argument only in the tenuous sense of showing that a certain legislative act, enunciation of a principle, is formally valid and legitimate, that it does come under the jurisdiction or office of the person in question; it can show neither that that legislative act

was right or well advised nor that that person was the right one for that office. Furthermore, since in the type of institution that both Stevenson's and Hare's analyses of moral language make us think of, the judge and the legislator can be any human being whatever, it would seem that when in doubt about what is the morally right thing to do or what moral principle to adopt, I can ask any man and get the final answer. But, of course, I do not ask just any man, nor do I need to think that the answer I get is final. I want to make sure that the person in question possesses enough wisdom, intelligence and experience and that in the given case his judgment is not a mere whim, prejudice or blunder.

There is an element of truth in Stevenson's and Hare's views. We saw that there is no set of rules or standards which specifies what must be the course of argument in defence of an approval in a test case, that is, in a case where the force of the utterance 'I approve of x' becomes 'I hereby subscribe to or enunciate the principle P'. The nature of such arguments, we found, was best described by saying that here the speaker must answer any specific challenges that are in fact brought forward. There is no way to tell, no general formula for telling beforehand and for certain, what these challenges ought, or are going, to be. The burden of proof lies, as it were, with the challenger rather than with the speaker. In a minimum way we all, as human beings, are qualified moral judges and legislators; *prima facie*, that is, unless we are challenged, we are all free, have the right, to prescribe to our fellow beings. To speak of the human prerogative to have emotions and attitudes and of the human predicament of having to make decisions is to point out the two aspects of this freedom. But we must not be carried away. Our ability and competence to enunciate norms is often challenged and many of these challenges are obvious and always in the air. As human beings we have a certain right to determine how others should act. In a minimum, and in a rather special, sense the justification of our moral judgments therefore consists simply in insisting on our feelings, attitudes and decisions. But as a human being I have the right merely to have my conscience heard; in that capacity I can claim nothing more. With a moral judgment I enter my case: to this as a human being, I have the right. In the further task of defending it, it is no longer sufficient merely to appeal to that right. I must, when challenged, go on and try to justify my judgment by a moral "proof"; and if that fails, by showing that I was in the position, had the competence, to pass it.

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## IV.—ON RELEVANCE

By GERSHON WEILER

IF I make a point, while engaged in an argument with you, it is a legitimate counter-move for you to say 'we are not interested in that' implying 'within the framework of our present discussion the point made by you is irrelevant'. It is possible to interpret this reply in the following way. There are separate frameworks of discussion and the appropriate notion of relevance in each, *i.e.* what arguments are permissible, legitimate, to the point etc. is determined by the given framework of the discussion. I want to examine whether this is the correct account of the matter.

The view under consideration derives its plausibility from a great many ordinary cases in which either the relevance or the irrelevance of a certain point is immediately obvious. In the moral evaluation of an action the intentions of the agent are obviously relevant while the clothes he was wearing, when performing the action, are normally not. In discussing the general trends of the market in consumer goods next year it is obviously relevant what the income of the average person is, while it is irrelevant whether he likes his coffee very hot or slightly cold. In these cases we feel confident that we know what we are talking about and this entails our knowledge of the appropriate criteria of relevance. Here we do not have genuine disagreements about the criteria of relevance, though we may have doubts on occasions. If we doubt whether a certain consideration is relevant or not, we feel it sufficient to fix our eyes on the concept or concepts underlying our framework of discussion; and the correct answer then immediately suggests itself.

It is quite different when there is a genuine disagreement about the criteria of relevance fitting the case on hand. These genuinely doubtful cases are also fairly common. In assessing morally that Mr. X did not return a sum of money he borrowed from Mr. Y on the day he promised he would, there is likely to be a genuine disagreement about whether Mr. X's present financial situation and other commitments, *e.g.* to his family are relevant to the evaluation of his not keeping a promise. There might be a genuine disagreement about whether in placing candidates in an order of merit for a term's work, punctuality, reliability or steadiness should be taken into account or not. The disagreement is about the relevance of these characteristics to the case on hand.

Then there is a type of case which is again different. There are

people who claim that the constellation of the stars at the moment of my birth is relevant to the question of what course my life is going to take, while others claim that this factor is totally irrelevant. This sort of case is frequently taken as the standard case in discussions about relevance and consideration of examples of this sort suggests a tie-up between the notion of relevance and that of causality. I shall return to this type of case towards the end of the paper.

How are we to account for our examples? Those cases in which there is no genuine disagreement about the criteria of relevance belonging to the discussion seem to suggest that *within* each framework of discussion there are certain fixed criteria of relevance. One may even be tempted to regard other, more doubtful cases where relevance is debated, as parasitic ones. I would like to suggest that the truth is just the opposite.

Take again the simple and clear-cut cases. We will find that there is a difference between frameworks of discussion in which all questions about relevance are settled while others are open to discussion. Now in the first set of cases, *i.e.* where all questions concerning relevance are settled, the very notion of relevance is redundant. In a case where I teach you a certain subject *S* (and *S* is such that all questions of relevance are settled concerning it) then my telling you that in *S* factors *x*, *y* and *z* are relevant is my teaching you *S*. Learning the appropriate criteria of relevance here is, at least part of, learning *S*. Our imagined subject *S*, *ex hypothesi*, is one with clearly fixed rules and consequently with clearly fixed boundaries. Such are arithmetic or the legitimate moves of chess. The notion of relevance is redundant here because all I could want to say by using the notion of relevance I could say equally well, or much better, by using the notion of 'rule'. Strictly speaking, no question concerning relevance can arise in such context. We are forced, then, to conclude that the home of the notion of relevance is in those cases where a genuine argument about relevance is possible. There is a logical tie-up between 'relevance' and 'argument about relevance'. Without the experience of the latter we would not have the former. The reason for this is that whenever we say that *a* is relevant to *b*, then *b* is a question or a problem. No consideration can be relevant to a fact, in so far as it is no more than a fact. But once some question arises concerning the truth of a factual statement we might be obliged to pass judgements about the relevance of certain considerations. It is part of the argument of this paper that the tie-up between *relevance* and some *question* is logically necessary. The argument will be complete once I have shown

what is the relation between the notion of relevance, the question involved and a discussional framework.

It should be noticed that just as the notion of relevance is redundant in cases where all possible questions concerning it are definitely settled, similarly in cases where no criterion of relevance has been settled, *i.e.* at the outset of the discussion there is no agreement about any single criterion of relevance, there too no question concerning the relevance of a certain consideration can arise. In these cases we simply do not know what we are talking about, or else we are not talking about anything in particular, we are certainly not having a discussion. The interesting cases will be, consequently, those in which some criteria of relevance have been settled while others are under dispute. In the examples I gave above, what is to be decided is the relevance of Mr. X's circumstances to his failing to keep his promise and certain traits of character in the case of the candidate who is being assessed. In both these cases we have a great variety of considerations which we take as relevant without further discussion. We do not ask whether his giving a promise is relevant to the question of the forgiveability of his not keeping it nor whether academic achievements are relevant to the placing of a candidate. These last mentioned considerations are regarded as obviously relevant.

Take another, more fanciful, example. We generally regard as irrelevant in assessing the moral worth of some behaviour what clothes the agent was wearing when doing what he did. Suppose now, that someone points out that the group the agent in question belongs to observes the prohibition contained in the Old Testament according to which no person should wear simultaneously garments made of wool and cotton. The reason for this prohibition seems to have been that farmers were supposed to be distinguishable from shepherds, and someone who paraded both kinds of garments was thought likely to be a false, unreliable and socially unsettled person. Should we not say, in talking about a cotton-wearing character's action, that his clothes were relevant? 'He was acting according to the sacred custom of farming folk.' Or 'He is only a farmer and consequently could not have known better.'

These latter examples all point in the same direction. If our framework of discussion is partly defined, *i.e.* it is obvious which considerations will count as relevant within its boundaries and partly undefined, *i.e.*, there is room for argument about the relevance of certain other considerations, then a discussion about relevance is necessarily a discussion about the proper boundaries of the discussion itself. An argument about relevance affects the

whole framework of discussion. The fact that we begin by taking for granted some criteria of relevance should not mislead us into thinking that they are not debatable. On the contrary, the very fact that debate is possible shows that all criteria of relevance can be objected to in their turn. In our examples the disagreement about Mr. X's negligence is not only a disagreement about the relevance of a certain fact (his circumstances) to the moral evaluation of his deed, but it is also an argument about the nature of moral evaluation itself. In the case of the candidate whose placing is under discussion, the argument in favour of putting him higher or lower on the list could be put in terms of the aims of higher education. The opposing parties hold different views on this subject (say, professionalism versus the ideal of the all-round boy) and their disagreement about relevance is in fact a disagreement about the subject, what is education? The difference between these and the first discussed clear-cut cases can be brought out also by saying that here we cannot refer to rules as, in a sense, it is just the rules which are disagreed upon : we make them up as we go along.

We might say, then, that all arguments about relevance are arguments *about* our frameworks of discussion. The cases in which it seemed that there is an argument about relevance within a framework of discussion were found to be not genuine. Yet, I have to account anew for the plausibility of the view that these arguments are possible. Or to put it differently : If it is obvious that the notion of relevance is redundant in the cases where the criteria of relevance are in fact contained in the concept of the subject-matter, how does it come about that we are inclined to think of arguments about relevance as arguments within the framework of a discussion? How does it come about that analysis is needed to show that in fact arguments about relevance are arguments about a framework of discussion? In short - Are arguments about relevance within the framework of a discussion possible at all?

Our previous discussion suggests the answer. No, arguments about relevance within a framework of discussion are not possible because (1) in cases where the subject is clearly defined the notion of relevance is redundant, and (2) in cases where the subject is not clearly defined any argument about relevance amounts to an argument about the subject itself. Why do we then incline to believe otherwise?

The answer to this question is to be found in the nature of language. The terms we use to refer to our conventional frameworks of discussion, *e.g.* morality, politics, education, love, achievement,

etc are notoriously imprecise. Subjects like arithmetic and the legitimate moves of chess are exceptions. The terms which we generally use to refer to our frameworks of discussion are different not only in so far that they have open boundaries or more exactly: the boundaries of the concept used to refer to the subject of the discussion can be opened, on principle, anywhere, but also in the respect that within that vaguely defined area there are many sub-areas which can engage our attention on occasion. As we cannot help using descriptive terms to refer to our frameworks of discussion, there is always room for attempts to clarify the boundaries of these, either from the outside or from the inside when we want to get clear about a sub-area of our subject. In the last mentioned case we are easily misled by the fact that the most general term we use to refer to our subject covers the whole area of our disagreement, and consequently we come to believe that our argument about relevance is within a certain framework of discussion. In a sense this is true. But the more important aspect of the truth of the matter is that the very same argument is *about* a part of what is generally regarded as one framework of discussion. In the case of Mr. X, say, it might seem that the whole argument is within the boundaries of ethics. To some extent this seems correct, only too much attention to this aspect of the matter might make us forget that it is mainly about promises, and yet whatever conclusion we come to will effect the notion of ethics itself.

Our areas of discussion, for which we have conventional referring terms, are in turn divided into sub-areas with equally ill-defined boundaries, and consequently the criteria of relevance differ in complexity. Suppose I want to buy a puppy. Someone says to me that I should not buy a shy one. I may ask here, how is the shyness of the puppy on this occasion when I, a stranger, approach it for the first time, relevant at all. He will then point out to me that healthy puppies are friendly and shy ones become nervous dogs when they grow up. What is pointed out here is a causal relation and in a case like this we have, on the face of it, something like an exception to the thesis I have put forward. It would be unnatural to say that the realization of a new factor (shyness) changed the boundaries of our framework of discussion (purchase of a puppy). The reason for this is that the consideration, the relevance of which was pointed out to me, was already entailed by a more general consideration to which I adhered all along, *i.e.* to buy a puppy which will grow to be a healthy dog. This last-mentioned consideration indeed was one of the things which determined the framework of our discussion



at the outset The case is similar to that of astrology which I mentioned earlier Generally we believe that the future of a man cannot be predicted except in the most general terms, when these predictions are based on past experience. They certainly do not include predictions of definite events, accidents and catastrophes. Those who claim that the constellation of the stars at the time of my birth is causally connected with my fate in the future, do in fact suggest that we base our predictions on the study of constellations instead of on intelligence-tests or whatever other modest means we might have for collecting data for our inductive generalizations. By debating whether to admit constellation-charts as relevant or not, we are debating the boundaries of the discourse concerned with the predictability of the future. Astrology is a rather exceptionally clear case it shows how a whole way of discussion depends on the acceptance of a single consideration as relevant

Our frameworks of discussion are mostly referred to by imprecise descriptive terms and this fact necessitates now and then the clarification of the concepts involved This clarifying enterprise can be seen to be performed once from within and another time from without That the arguments used in these clarifications appear in different lights at different times is due only to our linguistic conventions which leave the boundaries of our frameworks of discussion open to further attempts at precision and simultaneously create the illusion that these frameworks of discussion 'are there' given

There remains only to remove some possible misunderstandings concerning the notion of 'framework' itself and to clarify the relation of the question, which we found to be involved in all discussions of relevance, to the framework in which it occurs I am aware that 'framework' may have been an unfortunate choice of word inasmuch as it might suggest more precision than I intended the word to have. 'Framework of discussion' is not meant to be more than the most general answer to the question 'what are we discussing?' I have pointed out that the answers to this question will be extremely varied in precision and it was just this variety in the degree of precision which generated the problem which I have tried to solve in this paper But the main point is that as long as there is an answer to the question there is a discussion in connection with which considerations of relevance might be in place.

Of course, not all talk is discussing There is what Malinowski called *phatic communion* or what is generally called small-talk In such context questions are asked and answers are given, yet it

would be wrong to think that there is a 'framework' present in the sense in which the term has been used in this paper. The questions there are free-floating and they do not belong to any special framework, though they may come to belong to any specific framework of discussion by being asked in the right context. As long as questions remain free-floating they do not occasion any consideration of relevance 'Did you feel very dizzy?'—is a question with quite different consequences when asked in a court of law or during a casual conversation after dinner. Different tokens of the same question-type, when they occur either as free-floating ones or in frameworks of discussions which are very far removed from each other, should be regarded as different questions. That they are different questions could be shown by pointing out either that the appropriate criteria of relevance are different or that the framework of discussion is different. It has been the argument of this paper that it is impossible to point out either of these things without involving the other—they are logically tied together.

The original account I have set out to refute amounts to the belief that all possible frameworks of discussion, and all points of view, are given to us *a priori*, the former determining the latter. What that account fails to do is to provide an explanation for a change in our point of view. In fact this is one of the most important factors in our intellectual progress. Through discussing criteria of relevance we change not only the frameworks of our discussions, but also we introduce, on occasions, completely new frameworks of discussion. This new framework of discussion then acquires a name, which might exhibit the same lack of precision discussed above with the necessary consequences that it will invite further changes in our conventional frameworks of discussion. I have tried to show what is the linguistic background against which these changes occur.

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## V.—MEANING AND RULES OF USE

By ROBERT BROWN

"Don't ask for the meaning; ask for the use" is not always poor advice. Whether it is or not depends upon the problem to which it is offered as a solution. If someone says of a word or phrase, "We have a use for it in our natural language" or simply, "The expression has a use", his terminology is not as irresistible as he may think it is; nevertheless, it may help him to avoid the less elegant errors, *e.g.* the identification of the meaning of a word with the things to which the word can refer or to which it can apply. Having profited from the advice to this extent, its recipient may be encouraged to believe, without qualification, that "the meaning of a word is its use in the language".<sup>1</sup> From this point on there is a route paved with familiar arguments.

No. 1. You cannot really intend to say what I take you to be saying. To claim that an expression has a use is to claim in addition to other claims that it can be purposefully employed. But I may have an odds-bodds collection of noises which I use indifferently to frighten away cats and sparrows, to pronounce over witches' brews when I tell stories to my children, and to interject when I trip on the rug. They differ from the "Ouch!" of pain and the "Oh, oh" of surprise in that I seldom make the same noises twice or in the same order. They serve my purpose even when it is only to baffle people. If doing these things with noises is using them, surely my purposeful utterance of these noises does not give them meaning in our language?

No. 2. In order to distinguish among different kinds of use, and so answer your question, we say that "Any sound or mark can acquire meaning provided that rules are given, whether explicitly in definitions or implicitly by usage, determining its correct employment".<sup>2</sup> Thus to ask for the meaning of a word is simply to ask for the rules which regulate the use of that word.

No. 3. Similarly, we can say that expressions of which words are the elements, *i.e.* sentences, will be meaningful when the rules for the correct combination of the admissible elements have been followed.<sup>3</sup> In any particular instance these rules or their absence will allow us to decide whether the expression is meaningful or not.

<sup>1</sup> Wittgenstein. *Philosophical Investigations*, i. 43.

<sup>2</sup> J. L. Evans, "On Meaning and Verification", *MIND*, January 1953, p. 9.

<sup>3</sup> Evans, *op. cit.* pp. 15-16.

The view that the meaning of a word is given by rules for its use has been widely supported. The view that sentences have meaning whenever the appropriate rules of combination are observed has been less generally maintained. Yet, curiously enough, it is almost always emphasised that a word rarely occurs by itself. Whether heard, read, written, or spoken, words are usually found in groups. "Words do not occur in isolation except in certain special cases, e.g. swear-words and in dictionaries, and it is significant that a good dictionary, in giving the meaning or meanings of a word, gives examples of how the word is combined with other words in sentences." Thus "it is only in the context of a sentence that a word is meaningful".<sup>1</sup> On some interpretations, at least, the rules for the use of a word have also to be rules for the combination of that word with other words. If this is so, then the conclusion that rules of use should permit us to determine whether a sentence has meaning becomes difficult to avoid.

Now our chief question is: "Do linguistic rules always permit us to distinguish between sentences that have meaning and sentences that have not?" In answering this question it is also necessary to weigh the qualifications of certain applicants for the role of linguistic rule. The simplest procedure, then, is to consider, in turn, the credentials of three candidates which come to us warmly recommended. They are: grammatical rules, presuppositions, and definitions. They will be taken up in that order. First, however, an extended qualification of the Meaning-Use thesis as it applies to one class of natural languages will be offered. Then, after grammatical rules, presuppositions, and definitions have been discussed in connection with the main question of this paper, we shall have an epilogue in the form of a comment on the dictum. "the meaning of a word is its use in the language."

### 1 *Qualifying the Meaning-Use Thesis*

In a vast number of languages, especially the American Indian languages, the distinctions among word, phrase and sentence are exceedingly difficult to draw. In the polysynthetic languages, such as those of the Eskimo, Algonkin, and Athabascans, it is misleading to speak of *words* in the sense familiar to speakers of the Indo-European languages or of Chinese. The "minimum free utterance"<sup>2</sup>—one that can occur independently—is almost

<sup>1</sup> Evans, *op cit* p. 8.

<sup>2</sup> Cf. C. C. Fries, *The Structure of English*, 1952, chap. 2.

always a phrase or a sentence, depending upon the language. For this reason the question of what, in these languages, is to be counted as a word (in the European sense) is a highly debatable one among linguists. Examples that will illustrate the point can be arranged in an order of increasing complexity. Thus even in the Melanesian languages, which are not polysynthetic to any extent, there are the forms "tamagu" (my father), "tamamu" (thy father), "tamana" (his father). But the form "tama" cannot occur alone, and so there is no way of merely saying "father". In the Turkish form which can be crudely represented by "Komshulorımızinkilerdedirler" only the form "Komshu" (neighbour) can occur alone. The entire form is given only as a response to a question, and means "They are in those of our neighbours" (where "neighbours' gardens" is understood). In these cases it is not possible to define "word" by obtaining the repetition of part of the utterance in the way in which we can ask "Did you say 'his' or 'her'?" The complete utterance must be repeated. This is even more generally true, of course, when languages like Eskimo are being referred to, for in drawing a distinction between words, phrases, and sentences the New World languages are at the opposite end of the range from Chinese and English. This can be illustrated by examples drawn from West Greenlandic. In this language an utterance consists of a stem, usually the first syllable or the first two syllables, to which affixes and endings can be added indefinitely. A stem such as "nuna" (land) or "uvdloq" (day) is seldom used independently, each utterance is a complete one. Thus "nerdlerpai" means "He fed them", but the expression "nerdlerpai" cannot be separated into parts called "words". The same is true of more complex expressions "anlisa ut iss'ar siwu arpu ya" means "I am desirous of obtaining something suitable for a fishing tackle". Linguistic analysis shows that the element "ut" gives the sense "instrument", "anlisa ut" that of "fishing instrument", "siwa ya" of "obtaining my", and so forth. These are not words in the European sense, however. They have no meaning for the native speaker unless they occur within certain other expressions. A speaker of West Greenlandic cannot sensibly answer the question "What does 'ut' mean?" It does not mean anything except to the linguist, any more than "ible" (as in "irresistible") means anything to the ordinary speaker of English. A West Greenlandic-English dictionary has to list such elements as "ut" as independent items in the same way that "instrument" is listed in an English dictionary. This is a linguists' device, however. The manner in which "ut" and

"instrument" function in the two languages is quite different. "ut" is more like the English suffix, *e.g.* "ary" (in "honorary" the suffix means "pertaining to", but in "dictionary" it indicates location). This can be emphasised by considering the case of the Australian language, Nunggubuju.

In "Nunggubuju words generally present some such complex shape as, *e.g.* *nganujalwanagangmadji* 'if I touch his clothes' formed on the base *-wanaga-* 'touch, lay hold of, grasp'. Such a complex is here defined as a word because it can be used in isolation, whereas neither *-wanaga-*, *-madji* or any other part of the complex can be used. Even *-jahl-* 'clothes' is hardly a "word" in the given sense, because while its literal equivalent 'clothes' may be isolated in English, *-jahl-* normally needs at least one prefix to make it isolable. Even when lexical equivalents are given, as 'man' = *nawaljanjun*, the English element is an isolable word, and so is the Nunggubuju, but the Nunggubuju word consists of three parts. *na-walja-njung* the base is *-walja-* 'human', of the other two elements *na-* and *njung* both serve to define this 'human' as a single male specimen."<sup>1</sup>

Similarly, although the element "tama" has the meaning "father" when certain suffixes are given with it, "tama" is not a Melanesian word any more than "lasciv" in "lascivious" is an English word. To take the syllable "de" from the Turkish sentence quoted and say that in its context it has the meaning of "in" is not to say that "de" occurs with this meaning in a Turkish-English dictionary. The difficulties arising from the attempt to distinguish among words, phrases, and sentences in the polysynthetic languages result in a situation in which the point of these distinctions simply disappears. Linguists can, if they wish, call the utterance-unit either "words" or "sentences." But in either case the unit will be different from those found, for example, in the Indo-European languages. Thus instead of saying that the Greenlanders have no words we can say that they have no sentences. "Of course the Greenlanders employ a sequence of words in their talk, but these sequences can only improperly be called sentences. Apart from some few instances it may be said that the Greenlandic word contains in itself all the elements necessary to convey a complicated meaning, thus both noun and verb, both subject and object, where such are necessary.

*E.g.* *muvdluarquravtigit*, we bid you farewell  
*gajaqartuniarpara*, I am thinking of procuring a kayak for him."

<sup>1</sup> Quoted from a manuscript of Dr. A. Capell, University of Sydney

"Hence the Greenlandic word is the chief object to consider if we would understand the Greenlandic language. It is the word with which the grammar principally deals. And the word must not be regarded as a finished unit ready to be placed in and again fetched out of a dictionary. The Greenlandic word is living, is constantly coming into existence, so to speak, constantly in course of construction. Starting from a fixed point of departure, the stem, it is amplified more and more by additions of various kinds, as the meaning may require, until everything has been said that can and should be said by it . . ." Again "When several words are joined together to form a context corresponding to what we call a sentence, the words are not as in our language welded together to form one unit, but must rather be said to be co-ordinate, each single word—except the relative forms—having its own independent and quite complete (finished) meaning so that it can be understood by itself. Therefore the joining together of several words in reality only means a complement, an amplification, an explanation of the single word"<sup>1</sup> In this passage what the author calls a "word" we should be inclined to call a "sentence", and when he speaks of joining together several words we should speak of joining independent clauses to form a complex sentence. But this merely emphasises our point, of course

It is a mistake, then, to say what Mr Michael Dummett has said: that Mr. Strawson has given us a fantasy in suggesting that there could be a language whose sentences were not divisible into words. Mr. Dummett's argument runs like this: "try to envisage someone expressing in that language the thought that no one knows whether there is an odd perfect number or explaining to a child that the world is round. The idea seems plausible at first sight only because we think of extreme cases of what Frege called 'incomplete sentences' (sentences whose truth value varies with the occasion of their utterance). . . . Sometimes, too, it is argued that the sentence is primary on the ground that we can learn the meaning of a word only by learning the meaning of the sentences in which it occurs. But though it is certainly true of *some* words that we can learn their sense only by learning the use of representative sentences containing them, conversely there are some sentences—e.g. 'I expect Jones will resign within the next month'—whose sense we could not be taught directly,

<sup>1</sup> Both quotations are from Schults-Lorentzen, *A Grammar of the West Greenland Language*, Copenhagen, 1945, pp 13-14, and pp 95-96 respectively. For the other examples and for help on linguistic points I am indebted to Dr Stefan Wurm, Australian National University.

which we understand only by already knowing the meanings of the constituent words. Any attempt to express clearly the idea that the sentence is *the* unit of meaning, or even the idea that the meaning of sentences is primary, that of words derivative, ends in implicitly denying the obvious fact—which is of the essence of language—that we can understand new sentences which we have never heard before.”<sup>1</sup>

Clearly, the confusions in this argument could be removed only by prolonged treatment. They rest, however, on several mistaken assumptions. One is that all languages have words and sentences which are sharply distinguishable. Another is that “the unit of meaning” must be either a word or a sentence. A third is that the phrase “unit of meaning” is unambiguous. Is “ary” in English a unit of meaning? Or is “functionary”? What is the unit in a polysynthetic language? Finally, the problems of explaining to an Eskimo child that the world is round, or to an Eskimo adult that “no one knows whether there is an odd perfect number” are the obvious ones, and have nothing to do with trying to find a stock Eskimo expression for either of these views. In a polysynthetic language it is much easier than in any other kind (such as an isolating one) to form new expressions. But if there is no way of expressing the notion of world as distinct from Greenland, and no way of expressing the notion of round as distinct from something round, as an ice ball—for there are no adjectives in Eskimo—then to say “Greenland is round (like an ice ball)” will not quite do.

In those languages which do not distinguish words from sentences there is no unit that has all the relevant features of a word in, e.g. an Indo-European language. Hence, we cannot apply to the polysynthetic languages the formula “the meaning of a word is its use in the language” while at the same time maintaining that the meaning and use of a sentence are not identical, and that while there are rules of use for words there are none for sentences. We shall be forced to admit that sentences can be used in exactly the same sense that words can be used, for sentences and words will be identical. Again, we cannot say of the polysynthetic languages that certain of their expressions, e.g. sentences, are formed by the correct combinations of admissible words (where words and sentences are distinguishable in kind). Of course linguists classify and describe the elements of utterances for their own purposes, but these are not those of the ordinary speaker. It may be that supporters of the view that there are rules of use will wish to substitute some other expression

<sup>1</sup> “Nominalism”, *Philosophical Review*, October, 1956, p. 492.



for "word" in their claims and thus try to make them applicable to any language. But until this is done the claims can only be taken as applying to languages in which the distinctions among word, phrase, and sentence are present. This is an important qualification, for it is not clear whether it will be said that the temptations against which the identification of meaning and use is designed to protect us also present themselves in polysynthetic languages. If "ious" (as in "serious") is not to be found in an English dictionary do we wish to argue that the meaning of a suffix is its use in the language? Or do we wish to claim that only words, phrases, and sentences have meaning? Unless we make the latter claim we shall have to admit forms like "es" (plural), "'s" (possessive), "ing" (suffix) also possess meaning. But since these elements have neither sense nor reference by themselves, and cannot occur alone, it will be a question whether we are to say (1) that their meaning and use are identical and that in this respect they resemble words but not sentences, or (2) that they have uses but no meaning in the sense in which *any* word has meaning. However this situation is described, there will be an absence of certain temptations in those polysynthetic languages whose utterances must be translated into English by means of sentences. One familiar absentee is the view that the meaning of an expression is what is named by it, another is the assimilation of definite descriptions to proper names, a third is the assimilation of sentences to such names. There are others, of course. If these temptations are missing the work which the Use and Meaning thesis performs is considerably lightened. In restating it, supporters will have to show that the thesis earns its keep in *all* languages.

## 2. *Grammatical Rules*

Few people, perhaps, will wish to maintain that in the kinds of natural languages which clearly distinguish words from phrases and sentences, a sentence cannot be a rule of use (or a rule of combination) unless it is also a rule of grammar. Many people, though, may wish to maintain that some grammatical rules are rules of use. The most obvious problem is that of deciding which sorts of grammatical rules these can be. Suppose it is said that "Learning the language involves learning what grammatical type of sentence we must or may employ for what purpose, *e.g.* what type of sentence for asking a question, what type to make a request or give an order".<sup>1</sup> We *may* interpret this as referring

<sup>1</sup> K. Baier, "The Ordinary Use of Words", *Proceedings of the Aristotelian Society*, in (1951-52), 55.

to one class of rules of use (or rules of combination), i.e. rules of sentence types. Then it seems that a place must be found for rules governing the intonation curve of a sentence. For even if we confine ourselves to English there are cases in which the only difference between a question and an exclamation is that indicated by a difference in pitch. Thus we have "Will she be pleased!" and "Will she be pleased?" Now is the sentence "For an exclamation extend the high pitch over several syllables before the final drop"<sup>1</sup> a rule of combination? (Presumably it is not a rule of use.) Assume that it is a combination rule. Then it is clearly not one of the sort which by itself will allow us to determine whether a sentence has meaning. If some other intonation curve is present the meaning of the sentence may become ambiguous, or the utterance may be out of place, but the sentence will not be meaningless. This conclusion is given additional support if it is correct to say, as many people have said, that "a sentence is meaningful if there are rules possible or in existence such that the words which compose the sentence *could* be used to talk about something".<sup>2</sup> It is difficult, furthermore, to see how violation of the rule of intonation can combine with the violation of other rules to render the sentence meaningless. As a member of such a conjunction the sentence describing our prospective violation could contribute nothing useful. This will become apparent as we consider what else has been claimed for linguistic rules. Of course if learning how to employ types of sentences does not include learning the appropriate patterns of intonation, obviously not all the relevant rules are being learned. In any case, the kind of grammatical rules for which we are looking, those whose presence determines the meaning or lack of meaning of a sentence, will have to be sought elsewhere.

A rather different problem arises in the case of words. In Mandarin Chinese there is the form "ma" which can mean nurse, hemp, horse, or grasshopper, depending upon which of four tones it is given. Is the sentence "Use 'ma' (with a falling tone) to mean grasshopper" a rule of use? Apparently it is, because it is a sound or mark for which a rule "determining its correct employment" has been given. Then some rules of use are definitions and not rules of grammar, unless definitions are to be classified as one kind of grammatical rule. It makes no difference to the present argument which classification is adopted, but it is simpler in practice to treat definitions separately from rules of grammar, and this is the course which will be followed here.

<sup>1</sup> Fries, *op. cit.* p. 163

<sup>2</sup> Evans, *op. cit.* pp. 14-15.

Many expressions are less errors of grammar than sins against idiom: "unconscious to", "oblivious to", "disagree from" are well-known examples of the "misuse of prepositions". To say "The other man is the one who disagreed from my conclusion" or "The Congress could occupy itself with no more important question than with this"<sup>1</sup> is in the first example to use the wrong preposition and in the second example to use a superfluous one. Neither mistake seriously disturbs the sense. And we can understand that "Pamela is making (instead of taking) a walk" and "Pamela is taking (instead of making) it hard"<sup>2</sup> are ambiguous rather than meaningless. Much the same is true of at least some mistakes in syntax: "Roughly speaking, all men are liars"; "The lovers sought a shelter, and, mutually charmed with each other, time flew for a while on downy pinions", "Being pushed unceremoniously to one side—which was precisely what I wished—he usurped my place". All of these, says Fowler,<sup>3</sup> illustrate the use of participles that are either unattached or wrongly attached. Such blunders create a difference in sense. It is the lovers who are charmed and not time; it was I who was pushed and not my usurper. If there are rules applying to the use of idioms and participles the rules may prevent obscurity. It is obvious, however, that their violation in itself does not produce meaningless sentences. "All men who speak roughly are liars" may be false but it is not senseless. It is probably not what the author of the original sentence wished to say, yet he managed to say *something*.

Nevertheless, there are syntactical errors which produce more than obscurity: they produce contradiction. "My eyes are more and more averse to light than ever" is an example. "You can be more averse than ever, or more and more averse, but not more and more averse than ever. *Ever* can only mean the single point of time in the past, whichever it was, at which you were most averse. But to be more and more averse is to be more averse at each stage than at each previous stage."<sup>4</sup> It may be objected, of course, that in fact no rule of grammar has been violated, that the criticism offered deals only with the sense of the words, so that if definitions are to be treated as distinct from grammatical rules, then so should all questions of meaning. This classification is likely to be resisted. For a grammarian can claim that the contradiction arises from the combining of a sliding scale ("more and more averse") with a fixed standard of

<sup>1</sup> Fowler, *The King's English*, p. 174.

<sup>2</sup> Cf. Baier, *op. cit.* p. 55.

<sup>3</sup> *Op. cit.* pp. 119-123

<sup>4</sup> Both the example and the comment are from Fowler, *op. cit.* p. 81.

comparison represented by the *than*-clause<sup>1</sup> We cannot employ both at once without self-contradiction, since we cannot be more and more averse to light than at the single point of time at which we were most averse. If we say there is only one such point we cannot also say that there is more than one such point. If rules of this kind are to be excluded from the province of grammar because they deal with questions of meaning, much else will have to be excluded. What remains will be of considerably less help in distinguishing between sense and nonsense. On any interpretation of the scope of grammar a large number of grammatical rules are justified only by the difference they make in the meaning of the expressions to which they apply, *e.g.* "Not in the least undeterred", "I am not sure that he might as well not have gone", "I have now seen him, and though not for long, he is a man who speaks with Bismarckian frankness". In this last example the *though*-clause, being part of the second co-ordinate, takes its subject and verb from that; hence the result suggested is that the man's frankness will not last for long.<sup>2</sup> This confusion of subordinate with co-ordinate clauses leads to a sense not intended by their author.

Some ungrammatical expressions, then, are ambiguous or vague. Some are self-contradictory, *e.g.* "He is our mutual friend". Others, like "Those sort of folks ain't nothing good" have an idiomatic sense. But some offenders against grammar offend against sense as well without being self-contradictory. Thus we have. "It was pleasure that I slapped him with." However, there are also curious constructions of a type familiar to everyone. "My hair brush seems unprincipled", "Hatred runs in octagonals" and "The Army is the regiment" are examples which are not open to the objection that they are self-contradictory because of bad grammar. And, whatever is the difficulty with them it cannot be claimed that they are set apart from ordinary sentences by their violation of grammatical rules. If at least some of them are meaningless then grammatical correctness, apparently, is not a sufficient condition for an expression to have meaning. Rules of grammar provide no criteria which *always* allow us to distinguish between sense and nonsense. Mr. Michael Shorter has made this quite clear " . . . one will only know how many parts of speech to distinguish if one has already decided which forms of words to rule out. Furthermore, even if one did arrive at one's parts of speech independently of considerations of meaning, one would still have

<sup>1</sup> *Ibid.*

<sup>2</sup> Taken from Fowler, *op cit* p 327

to know what statements are meaningful in order to know what rules to lay down in terms of these parts of speech. It is not grammatical rules that are fundamental but the meaningfulness or otherwise of forms of words. It is not necessary first to know what the rules are before one can pronounce upon the meaningfulness of a given form of words. Rather one has to know which forms of words are meaningless in order to decide if a given rule is a correct one or not."<sup>1</sup> Hence, a rule of grammar cannot be that kind of rule of use (or rule of combination) which always permits us to divide nonsensical sentences from those with meaning.

However, there is an objection to this conclusion. It is that the examples produced of grammatical but meaningless sentences are not genuine, that for any grammatical sentence a situation can be found in which it will have meaning. Only ingenuity is required. Someone accustomed to the Sunday ordinances called "Blue Laws" may find the step to saying "Virtue is blue" quite an easy one. And with the example of "But me no buts" to guide us, "Virtue is but" should not prove difficult. Even "Fetch me a quickly"<sup>2</sup> can find its place in a word game. The ready reply to this objection is, of course, that we have found meanings for the same forms of words but have changed the sense of the words. The meaning of the word "quickly" differs in the two sentences "Paula fetched me the bread quickly" and "Fetch me a quickly". Our task was to retain the meaning that "quickly" has in the first sentence and this was not done. "But" is neither identical with virtue nor is it an attribute of it as the form "Virtue is but" may seem to state. The person who utters the remark "Virtue is blue" is not claiming that virtuous actions come in colours. These examples remind us, though, that we cannot simply argue that certain grammatical sentences have no meaning. We must also indicate for each word which sense is to be preserved.

In addition, the phrase "grammatical sentence" is far from clear. It may be taken as (1) referring only to the word-classes and their arrangement. In English this gives us such information as the tense, the performer and subject of action, the number, and the sentence type. This information, and more like it, cannot be obtained by looking up the dictionary definition of each word in a sentence. The pattern embodied in "He fell over her feet" is quite different from that in "She falls over her feet?" The various devices which indicate such things as who is acting and time of occurrence may be responded to by an English speaker even

<sup>1</sup> "Meaning and Grammar", *Australasian Journal of Philosophy*, August, 1956, p. 79.    <sup>2</sup> Both examples are taken from Shorter, *op. cit.*

if he does not know the meaning of the words in the sentence. Or (2) "grammatical sentence" may be interpreted as referring to these structural devices of the sentence and to the dictionary meanings of the words as well. On this interpretation such utterances as these are ungrammatical: "1 Woggles ugged diggles 2. Uggs woggled diggs 3. Woggs diggled uggles." But if we take these as displaying the patterns of English sentences then we can legitimately infer from (1) the following: (4) "A woggle ugged a diggle"; from (2) there follows (5) "A ugg woggles a digg", and from (3) we can deny that there follows (6) "Uggles diggled woggs". We can do this because "woggles", "uggs", and "woggs" are placed in the position occupied by thing-words in English sentences. The endings of these words show us that they are in the plural. The second word in each of sentences (1), (2) and (3) is to be taken as a performance word, and the action (in the past) was toward things called "diggles", "diggs" and "uggles". In responding to the cues of these patterns the speaker of English is recognising the form-classes (or parts of speech) to which English words belong.<sup>1</sup> If grammar consists of the set of pattern-signals, then examples like "Woggs diggled uggs" are grammatical. But they are meaningless in that each form is only a proxy for an English word of a certain class. There is no situation in which "woggs diggled uggs" will have the kind of meaning envisaged by those who believe that any grammatical sentence can be given a meaning.

### 3. *Presuppositions and Definitions*

There is another interpretation often given to "linguistic rules"; it is that at least some of them are presuppositions. It has been argued, for instance, that violation of certain presuppositions of language-use leads to absurdities. This view is sometimes amplified so as to assert that abuses of this type reveal the presence of rules, and that the presuppositions of use are one sort of prescriptive rule. Take as an example of an absurd sentence, "I went to the pictures last Tuesday, but I don't believe that I did".<sup>2</sup> Of this we may ask: "Is it possible for a person to disbelieve what he in the same breath honestly asserts? Or is a sentence of this type a plain misuse of language because it violates one of the presuppositions of an 'honest assertion', the

<sup>1</sup> These "nonsense examples" and the linguistic points accompanying them are adapted from those of Fries, *op cit* pp. 70-72.

<sup>2</sup> Quoted from G. E. Moore by Max Black in "Saying and Disbelieving", *Analysis*, December, 1952.

pre-supposition that the speaker believes what he is saying ? And is the sentence absurd for this reason ? ”

Professor Baier has said<sup>1</sup> that if we interpret “ I don’t believe I did ” as “ I did not go to the pictures last Tuesday ” there is an obvious self-contradiction. If, on the other hand, “ I don’t believe I did ” refers to the speaker’s state of mind, then the original remark is not self-contradictory. It is “ *plainly* useless and *pointless*, and *inexcusable* ”, Baier says. What if we tried to *find* a use for this remark and other “ pseudo-assertions ” like it ; would this be “ changing the language ” ? Apparently it would be. Professor Black<sup>2</sup> emphasises the point by saying that if the speaker had good evidence to show that he went to the pictures yesterday but could not remember going to them, the speaker should then *have* to say, “ I suppose I must have gone to the pictures yesterday ”. This assertion would not be as strong as the original one.

Suppose a man is being treated by a psychiatrist and is under post-hypnotic suggestion. It may be part of his treatment that he perform some action and yet refuse to believe that he has done so, at least temporarily. After going to the films he is given the most complete evidence that he did go. He replies, “ There is no further evidence for which I can ask. It is clear that I must have gone to the theatre. But something is forcing me to withhold my emotional assent. Of course I am prepared to *assert* that I did go, just as after the death of my brother I did not deny that he was dead nor that his wife was responsible. Yet in the same way that I could not immediately reorganise my feelings, attitudes, and behaviour to take account of this situation, acting at times as though he were still alive and his wife not guilty, so I only half-believe that I went to the pictures. I have no recollection of going and it seems incredible to me that I actually went, although there seems to be no doubt that I did. In this strong sense of ‘ believe ’ I don’t believe that I did, although in a weak sense of ‘ believe ’ I do believe ”

People who regard the sentence about going to the pictures as chronically unemployable do so, they claim, because it violates a presupposition of honest assertion. But there is a wide range in the kind and intensity of belief. It may be fervent or perfunctory. It may shade into doubt. Which of these kinds is required for honest assertion ? If we argue that *any* degree of belief is sufficient, we admit “ I went to the pictures last Tuesday but I don’t believe that I did ” as a genuine assertion. We admit,

<sup>1</sup> In “ Contradiction and Absurdity ”, *Analysis*, December, 1954

<sup>2</sup> *Op. cit.* p. 28.

as well, many sentences of polite conversation. The rule will amount only to saying this: "it is a presupposition of a genuine assertion that the asserter believes it more than he disbelieves it, i.e. that he does not lie". In this case the presupposition will be the tautology: "an honest assertion requires that the speaker not be lying". If we argue that a strong degree of belief is needed for genuine assertion, we have to show that the excluded sentences are pointless. How can this be done? Not, surely, by claiming that the situation described in the example has not occurred in the past and will not in the future. The record of past usage and the prediction of future practice are not under debate. What we wish to know is whether an appeal can be made to a "rule of honest assertion" in order to bar the employment of an expression when it is that employment which brings the rule under criticism. No one can reasonably suggest that in the example used the speaker would be making a pointless remark. For his listeners would know what to expect of future cases and they would know how to explain them. Therefore, either the presupposition of honest assertion is tautologous and admits sentences honestly asserted but partially disbelieved by their speaker, or it excludes too much. (There are, of course, various kinds of conditions required for successful communication in any given instance, and honesty is often one of these conditions.)

An example of another kind is that which requires the subject classes to have members if sentences like "None of my dogs has fleas" are to be usable. This presupposition, also, can be phrased as a tautology: "If an expression is successfully used to refer, there must be something to which it refers". Other examples of presuppositions are what Mr. Strawson calls "referring rules". He says, "A referring rule lays down a *contextual requirement* for the correct employment of an expression".<sup>1</sup> Since referring rules are concerned with the rather general conditions under which an utterance is made, it is not clear whether the rule of honest assertion is to be placed in this category. Whether it is or not, the examples given of referring rules are of the following type: "the word 'I' is correctly used by a speaker to refer to himself", "the past tense is correctly used to indicate that the situation or event reported is temporally prior to the report".<sup>2</sup> Given the dictionary definitions of "I" and "past tense", these rules are tautologies. The interest of this point is that presuppositions are simply definitions. Is there any important difference between ignoring the definition of the word 'I' as is done in "I were muttering to myself", and violating the rule

<sup>1</sup> *Introduction to Logical Theory*, p. 213

<sup>2</sup> *Ibid*



that " ' I ' is correctly used by a speaker to refer to himself " ? Apparently not ; hence, in saying that the referring rule for " I " has not been obeyed, *i.e.* that the definition of " I " has not been adhered to, we have to decide whether it is this which is responsible for the sentence's lack of meaning. If it is, then a violation of a rule of use has at last provided us with an example of a meaningless sentence.

Suppose we were to assume that the complete definition of a word would also include a description of the grammatical rules which applied to it. Suppose, as well, that we understood the grammatical rules of a word to include its dictionary definitions. If we made these assumptions we could consistently assert that in the sentence " I were muttering to myself " the definition of " I " had not been preserved ; or we could assert that the grammar of the sentence was faulty. It would make no difference. The rules of use would also be rules of combination ; and the rules of grammatical construction would enable us to select those words with the appropriate powers of combination shown in their rules of use. If we took account of both definitions and grammar in this way, would violations of these joint rules invariably produce meaningless sentences ? Clearly not. A failure to preserve a definition in this extended sense of " definition " would still not have the result of always producing a meaningless sentence, *e.g.* " I were muttering to myself ". For a consequence of this argument would be that every time a word added or lost or changed a sense we should have to say that the original sense had not been preserved and that the sentence in which the word occurred was meaningless. The first person who said " The electric current *runs* along this wire " must then have been talking nonsense, and so must have the early Freudians when they spoke of unconscious desire. In each case there was a failure to preserve a definition, in the first case of " runs " and in the second case of " desire ". If in the future a person appeared who had multiple personalities which sometimes alternated rapidly with one another, the person might find it convenient to say " I was muttering to myself " or, on occasions when the personality changed during speech, " I (plural) were muttering to myself ". What this shows us is that even if we can explain in terms of the violation of a rule why a sentence lacks meaning it does not follow that we can employ that rule as a *criterion* for distinguishing sentences with meaning from those without it. The reason has already been given : we have to know whether the sentence has meaning before we can pass judgement upon the soundness of the rule. We change the rules, such as they

are, by our practice. Thus we derive the grammar of a natural language (in both senses of "grammar") as a description of the signals that turn up in people's speech, and the correctness of the description is tested by what people say and write. We cannot then employ the description as a criterion for sifting sense from nonsense. If to claim that "a sentence will be meaningful when the rules for correct combination of the admissible elements have been followed" is to contend that we can use the rules as such a standard in any given case, the contention is mistaken. If the claim is that whenever a sentence has meaning the combination rules derived from previous speech have been followed completely, then the claim is merely the converse of the previous one and is equally in error. And to argue that compliance with the correct description of the grammar will produce sentences with meaning is trivially true; but only if "grammar" is understood to include reference to the meaning of the words.

However, it may be pointed out that neglect of the definitions of words results in contradictions. Some rules of use are explicit or implicit definitions, and it is breaches of these rules that allow us to pick out sentences which are nonsensical in being either self-contradictory or in leading to contradictions. But even thus greatly weakened claim presents a difficulty. Take three examples of peculiar sentences: "Mr. 1957 is a happy man"; "Even a circle can fall in love"; "It was pleasure that I slapped him with." Assume that each of these leads to contradictions when its words are given certain of their dictionary meanings. Can we show that each sentence leads to contradictions merely by referring to the breaches of rule committed in it? Obviously not. We can provide a demonstration only if we know what related statements will be defended, *e.g.* that "pleasure" is not the name of a physical object but of an odd sort of thing. Without having this kind of information we shall not know what general point of logic, if any, is being attacked. Lacking that knowledge we can equally well interpret the sentence in question as being empirically false.<sup>1</sup> Thus no one will receive much aid from merely being told to examine the definitions of the words in a sentence in order to discover whether its employment will lead to contradictions. And in the case of the three sentences quoted, the assertion that they are self-contradictory is questionable, to say the least.

Whether we take rules of use to be rules of grammar, pre-suppositions or definitions, or all of these together, there is this problem to be faced. when the thesis that a sentence will be

<sup>1</sup> This argument is found in Baker, *op cit* p 21

meaningful when the correct rules have been followed is unobjectionable it is trivial, when it is interesting it is false. If we understand the conventions which are illustrated by grammar, presuppositions, and definitions to be a description of the living language, then it cannot be correct to say that it is the conventions ("linguistic rules") which always permit us to pass judgment upon the soundness of sentences. As we have seen, it is these sentences themselves which provide the standard for the correctness of our description. If, instead, we claim only that each sentence will be meaningful when the linguistic rules which it illustrates have been followed our claim will be vacuous.

#### 4. *The Meaning of a Word is its Use in the Language*

Is the meaning of a word identical with its use in the language? Is it true that "to ask for the meaning of a word is simply to ask for the rules which regulate the use of that word"? The sense in which "linguistic rules" are like principles, standards, guides, maxims, canons, precepts, laws, regulations, customs or conventions has been much debated. Even if we ignore this question, however, it is clearly incorrect to identify meaning and rules of use. When an English speaker asks, "What is the meaning of the word 'fabaceous'?" he can be correctly answered by "having the nature of the bean, like the bean". Which rule has he been given? Presumably the following. "The word 'fabaceous' is applicable to whatever has the nature of the bean or is like the bean". But the rule does not tell the speaker how "fabaceous" may be arranged with other words in sentences. It does not tell him whether he can say, "Was the woman fabaceous?" or "Fabaceous, she clung to the ladder." It tells him nothing of the grammatical role the word can play, into which idioms, if any, it can enter, what its emotive suggestions are. To ask for the meaning, then, is either to ask for all of these relations to other words, since they are part of the use of the word, or it is to ask for only some of the rules. According to the first answer, then, to ask for the meaning of a word may be to request a description of the entire language grammar, vocabulary, pronunciation (for this may change with different senses of the word), intonation in different contexts, and so forth. According to the second answer, the meaning consists of a portion of these rules. Which though? It cannot be those which the questioner happens to find useful at the time of inquiry. Nor can it be those dealing with grammar, pronunciation, and punctuation unless these rules are part of the meaning of the word. If they are not,

then the formula will read "To ask for the meaning of a word is simply to ask for the rules which regulate the meaning of the word" Short of objecting to the word "rules", we can have no quarrel with *this* formula

From the shortcomings of the original formula it does not follow, of course, that sounds or marks or gestures do *not* acquire meaning in a language by having rules provided for them. Saying that meaning is acquired by the provision of certain rules and that these rules constitute the meaning is different from saying that to ask for the meaning is to ask for the rules which regulate the use of the word. For the meaning of a word may be given by rules, but the class of meaning rules is only a sub-class of those rules which regulate the use of a word

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## VI.—CONJECTURES AND REFUTATIONS ON THE ONTOLOGICAL STATUS OF THE WORK OF ART

BY ROBERT HOFFMAN

IN this paper, I examine certain theses advanced in reply to the question, "What is the ontological status of the work of art?" Scrutinizing what has been written on the subject has convinced me that theories of the ontological status of the art work are rarely refuted; usually, they are merely opposed by counter-theses or shown to be awkward in some way or other. What I attempt to do is to state the theses (hence "conjectures") and then to refute them by demonstrating inconsistencies or other serious defects in them (hence "refutations")

Consider the following statements about particular works of art.

1. Shelley's *The Cenci* was dedicated to Leigh Hunt, Esq.
2. Shelley's *The Cenci* is shorter than his *The Revolt of Islam*.
3. Beethoven's *Op. 13* is shorter than his *Op. 111*.
4. Beethoven's *Op. 13* is in three movements
5. Beethoven's *Op. 13* expresses a youthful conception of tragedy.
6. Serkin's *Pathétique* is superior to Rubinstein's
7. Rubinstein's *Pathétique* is coupled with the *Appassionata*.
8. My *The Cenci* is torn.

Statement "1" refers to a series of marks on paper, indeed, to the first such series published under the name "*The Cenci* by Percy Bysshe Shelley" or under some synonymous designation, statement "2" refers to any and all sign-events of the sign-design, "*The Cenci*", statement "3" refers to (a) an ideal performance of that sonata, (b) the mean performance of it, (c) the mode performance of it, (d) the critically permissible mean performance of it or (e) the critically permissible mode performance of it; statement "4" refers to (a) the original manuscript of that sonata, (b) any accurate copy of that manuscript or (c) any critically acceptable version of that manuscript; statement "5" refers to (a) the projected experience of a particular listener, (b) the projected experience(s) of most listeners, (c) the projected experience(s) of most qualified listeners, (d) the projected experience of an ideal listener or (e) an ascribed regional quality of the object, conceived as a potentiality or a disposition to cause the experience(s) mentioned in "(a)" through "(d)";

statement "6" refers to (a) some particular performance by each of the pianists, (b) the usual performance by the pianists or (c) the best actual performance by them, statement "7" refers to (a) a particular recorded performance or (b) a performance at a particular concert, and statement "8" refers to a certain copy of the poem, i.e. to a particular sign-event. Which sort of the rather diverse sorts of object referred to by the statements listed above is the work of art? In other words, what is the ontological status of the work of art?

I shall begin my investigation by examining a theory sketched ever so briefly by Abercrombie, according to whom the work of art is constructed by and inheres in the spectator's self-consciousness<sup>1</sup>. Thus, a poetical work of art is understood as being the succession of experiences—sensations, images, feelings and ideas—that he has when he is reading the poem. The work of art is "the same thing as his self-consciousness".<sup>2</sup> But this view is faced with two overwhelming difficulties. First, since the experiences of different spectators who are confronted by the same object differ, there are on this view as many art works associated with a given poem as there are spectators reading it. From this it follows that no two persons ever do talk about the same work of art, for the work of art would be purely private. It would be an epistemological, not an ontological, object.<sup>3</sup> But, in fact, we do communicate when we talk about art works. When, on page one, I wrote "Beethoven's *Op. 13*", you were able thereby to know to which of that composer's works I was referring and should I go to a piano and begin playing another of his sonatas you will say, "No, that's not it"; and should I then begin playing *Op. 13*, you will signal recognition by saying, "Yes, that's the one". Second, how do the successive experiences constitute one work of art? Suppose, for example, that a person has the same succession of experiences when he reads Shakespeare's *Sonnet XXIX* as when he reads Shakespeare's *Sonnet XXX*. Should we say that there is but one work of art? To do so would be inconsistent with what we perfectly well do know, namely, that there are two sonnets, each a work of art in its own right. Nor can we escape the inconsistency by arguing that since the visual sensation caused by the printed page is different in the case of one sonnet from what it is in the case of the other,

<sup>1</sup> Lascelles Abercrombie, *An Essay towards a Theory of Art* (London Martin Secker, 1922), especially pp 42-43

<sup>2</sup> *Ibid* p 43

<sup>3</sup> Cf C D Broad, *The Mind and its Place in Nature* (London: Routledge & Kegan Paul Ltd 1925), especially pp 140-142

the same succession of experiences cannot in fact be had. For in that event, we should also have to say that a reader who thrice reads only *Sonnet XXIX*, but who reads it once in roman, once in gothic and once in cursive type and who therefore has different visual sensations at each reading, apprehends a different work of art each time. But this is plainly nonsensical: he is reading *Sonnet XXIX* and whether its sign-design or something associated with it constitutes the art work, surely the style of type is irrelevant to the ontological status of that work.<sup>1</sup>

Although we may not be able to equate the work of art with the experience(s) of a spectator(s), perhaps we can equate it with that of the artist who created it. Accordingly, we may say that the work of art is an imaginative experience had by the artist.<sup>2</sup> Thus, a poem is not what is printed on paper, the marks on paper are merely means by which a spectator, if he reads intelligently, can reconstruct that experience for himself. The poem that is enjoyed as an art work is never sensuously seen (or heard) at all, it is something imagined.

But this view is no more sound than the one we have just rejected. Since no one but the artist himself ever has his experience, to equate the poem with that experience means that only the artist himself can be said ever to be acquainted with the poem. To be sure, it may be suggested, *per contra*, that this objection is based on a too literal reading of "the artist's experience"—that although a spectator cannot have the artist's experience, he can have an experience very like the artist's in content. And to the extent that he does have such an experience, he too is acquainted with the art work. There are, that is to say, degrees of knowing a work of art, and only the artist knows it fully. But this reply merely replaces one difficulty with another. The reply states, in effect, that if and only if the content of a spectator's experience overlaps that of the artist's does the spectator know the art work. The more extensive the overlap, the greater the degree of knowledge. But in order to verify that there is such an overlap, we should have to know what the artist's experience is. Yet since he alone is in a position to know this, no one can say whether or not a spectator's experience does in

<sup>1</sup> Were this theory not unsound for these reasons, it would present an obvious difficulty, namely, that we should be uncertain as to whether the experience in question is that of (a) just any spectator at all, (b) any qualified spectator or (c) an ideal spectator, i.e. one who makes no mistakes. This difficulty is representative of the sort that suggests a certain awkwardness in a theory but does not refute it.

<sup>2</sup> R. G. Collingwood, *The Principles of Art* (Oxford: The Clarendon Press, 1938), especially pp. 139-151.

fact approximate his own. Nor can the artist himself say whether or not it does, for he is in the same position with respect to the spectator's experience as everyone else is with respect to his. Thus, on the view being criticized, we should be incapable even in principle of knowing whether or not someone has "read" a poem at all, to say nothing of whether or not he has "read" it correctly.

Can our rejoinder be obviated by arguing that the artist can tell the spectator whether or not the latter's statements allegedly describing his reconstruction of the former's imaginative experience are correct? Assertions by the spectator—so the argument runs—are *for him* about another mind, but the artist, about whom they are made, is able to say whether they are true or false because he can directly inspect his own imaginative experience and determine whether or not he does experience what he is claimed to experience. But there is a telling objection to this rejoinder. According to the theory in question, the artist's imaginative experience is itself the art work and the poem is merely the most accurate record of that experience. Consider these lines:

And on the water, like to burning coals  
On liquid silver, leaves of roses lay.

The spectator can describe his reconstruction of the poet's imaginative experience by saying, "You saw a scatter of red rose leaves floating on water". Yet this description, although not wholly so, is woefully inadequate. The poet would reject it and then quite sincerely and honestly describe his experience by repeating the lines in question. "When I write poetry", he might say in Humpty Dumpty-like fashion, "it means just what I choose it to mean—neither more nor less". And the spectator would thereby be reduced to repeating the poet's lines and hoping that they call up in his mind a vision exactly corresponding with the poet's. But because the content of each mind is private, he is doomed never to know whether his vision does correspond with the poet's. At best, by comparing his language to the poet's, the spectator can discover that they use the same words in all situations and that the structure of their languages is the same, but he cannot validly infer that their words signify their having exactly the same or even similar imaginative experiences. Hence, we cannot identify the work of art with the artist's imaginative experience.

At this point, another view of the ontological status of the art work recommends itself, namely, that the work is a construct out



of the spectator's aesthetic experience.<sup>1</sup> According to this view, we must distinguish three categories when we talk about an art work : there are (a) the physical vehicle, *e.g.* the poem printed on the page, that acts as a control object and determines certain limits within which fall, (b) the aesthetic perceptions or appearances that constitute the content of felt givenness had on stimulation by the control object ; and out of these perceptions is constructed, (c) the object of critical and interpretative evaluation or the work of art.

But this view raises several difficulties. First, since different persons construct the art work on the basis of different perceptions—the perceptions being private—it follows that each person in talking about a construct out of his own perceptions is talking about something capable in principle of being apprehended only by him. Moreover, if at time  $t_0$  I construct the work of art on the basis of perceptions  $p_1$ ,  $p_2$  and  $p_3$ , and at time  $t_1$  construct it on the basis of these perceptions and perception  $p_4$ , then I am talking about two different constructs and *ipso facto* about two different art works. Second, if the name of any particular work of art signifies merely a construct out of the perceptions had by someone on a particular occasion, then there is no subsistent art work. That  $x$  be a work of art would mean merely that there be some person  $z$  for whom, at some time  $t_0$ , being presented in an appropriate manner with the vehicle  $y$  (associated with  $x$ ) is a necessary and sufficient condition for  $z$ 's having an aesthetic appearance or a set of such appearances. The symbolization of this is :

$$Wx = \text{df } [(\exists y)(\exists z)(t_0) : ((\forall yx) \cdot (Pyz \equiv zA_1 \vee zA_2, \dots zA_n \vee \dots))] \quad (I)$$

The expression, "the work of art" would be an incomplete symbol, not a designator. Third, if the name of an art work refers merely to the perceptions had by someone on a particular occasion, then we should be unable to speak of right or wrong with respect to any statement of the form, " $x$  is a work of art". where " $x$ " designates a construction out of someone's perceptions and "is" is identificational. This is so because we should have no independent standard or criterion by appeal to which we could determine the correctness of such a statement. This has been pointed out by Wittgenstein in another context :

<sup>1</sup> A theory of this sort is proposed in Stephen C. Pepper, *The Basis of Criticism in the Arts* : "Supplementary Essay" (Cambridge, Massachusetts : Harvard University Press, 1949) and *The Work of Art* (Bloomington : Indiana University Press, 1955).

Let us imagine a table (something like a dictionary) that exists only in our imagination. A dictionary can be used to justify the translation of a word X by a word Y. But are we also to call it justification if such a table is to be looked up only in the imagination?—"Well, yes, then it is a subjective justification"—But justification consists in appealing to something independent—"But surely I can appeal from one memory to another. For example, I don't know if I have remembered the time of departure of a train right and to check it I call to mind how a page of the time-table looked. Isn't it the same here?"—"No, if this process has got to produce a memory which is actually *correct*. If the mental image of the time-table could not itself be tested for correctness, how could it confirm the correctness of the first memory? (As if someone were to buy several copies of the morning paper to assure himself that what it said was true.)

Looking up a table in the imagination is no more looking up a table than the image of the result of an imagined experiment is the result of an experiment.<sup>1</sup>

In the case in point, once the perceptions have passed, there is nothing to which to point to justify someone's statement, "X is a work of art". Nor can it be argued in rebuttal that he might have the same perceptions again. For as Wittgenstein writes (again in another context):

Let us imagine the following case. I want to keep a diary about the recurrence of a certain sensation. To this end I associate it with the sign "S" and write this sign in a calendar for every day on which I have the sensation—I will remark first of all that a definition of the sign cannot be formulated.—But still I can give myself a kind of ostensive definition. How? Can I point to the sensation? Not in the ordinary sense. But I speak, or write the sign down, and at the same time I concentrate my attention on the sensation—and so, as it were, point to it inwardly.—But what is this ceremony for? for that is all it seems to be! A definition surely serves to establish the meaning of a sign.—Well, that is done precisely by the concentrating of my attention, for in this way I impress on myself the connexion between the sign and the sensation.—But "I impress it on myself" can only mean: this process brings it about that I remember the connexion *right* in the future. But in the present case I have no criterion of correctness. One would like to say: whatever is going to seem right to me is right. And that only means that here we can't talk about 'right' (I. 259).

This shows that speaking of correct and incorrect identification

<sup>1</sup>Ludwig Wittgenstein, *Philosophical Investigations*, trans. G. E. M. Anscombe, 2nd edn. (Oxford: Basil Blackwell, 1958), I 265

of something as an art work would be possible if and only if the aesthetic object were somehow public, for only on that condition could there be an independent criterion. But, on the construct theory, this condition, *ex hypothesi*, does not obtain.

Professor Pepper also suggests an alternative account of the art work within the framework of the construct theory. For he writes that the work of art "is in the nature of a potentiality or dispositional property of the vehicle. It is the full potentiality of aesthetic perception available to the aesthetic vehicle."<sup>1</sup> Now, first of all, it is misleading to use the concepts *potentiality* and *disposition* interchangeably, for the former involves merely that there are possible conditions under which a vehicle will appear such-and-such, whereas the latter involves that there are certain actually *specifiable* conditions under which it will do so. I shall assume that he means that a particular work of art is a specific dispositional property of a particular vehicle and that, speaking generally, the work of art is a potentiality of the vehicle to cause aesthetic perceptions.<sup>2</sup> Thus, individual control objects have the same potentiality by virtue of having different dispositions.

Although this view is somewhat more sophisticated than those we have already rejected, it is no more acceptable than they. It asserts that the art work is the class of potential aesthetic perceptions caused by the vehicle. But if we hark back to "(I)" we notice that the class of such perceptions or appearances is open; its membership is never fully determinate. It follows that we can never know "the full potentiality of aesthetic perception available to the aesthetic vehicle"—that we can never know the work of art. But to say that there is a work of art implies that it is knowable, i.e. is an object of knowledge, for an assertion about existence leaves one open to the question, "How do you know?" This question is unanswerable here, for we are in principle incapable of knowing the full potentiality of the aesthetic vehicle, and it is with the full potentiality that the work of art is equated.

A view similar to the first account suggested by Professor Pepper has been advanced by Professor C. I. Lewis in his *Carus Lectures*.<sup>3</sup> An art work, according to this view, is an abstraction

<sup>1</sup> Stephen C. Pepper, *The Work of Art*, pp. 30-31.

<sup>2</sup> Cf. Wilfrid Sellars, "Aristotelian Philosophies of Mind", in Roy Wood Sellars, V. J. McGill and Marvin Farber, eds., *Philosophy for the Future* (New York: The Macmillan Company, 1949), especially pp. 545f., and C. D. Broad, *Examination of McTaggart's Philosophy* (Cambridge, England: The University Press, 1933), I 264-278.

<sup>3</sup> Clarence Irving Lewis, *An Analysis of Knowledge and Valuation* (La Salle, Illinois: The Open Court Publishing Company, 1946), especially pp. 469-478.

actualized by presentation through the medium of some physical vehicle. But the abstractness, contends Lewis, is not like that of triangularity or honesty or incompatibility: "It has the literal character of esthesis"<sup>1</sup> But in what sense can the abstraction have that character? Although the conceptual element of the abstraction is at a minimum—in contrast to what it is when we are concerned with triangularity or honesty or incompatibility—the interpretative and constructional elements are prominent. Indeed, Lewis himself recognizes that the interpretative element of the abstraction is prominent, for he remarks that "the aged man and the child by his side may both read the same responses from the prayer-book, but these words cannot have the same meaning for the two, because in the one case they are freighted with a lifetime of experience"<sup>2</sup> But he passes off the difference by insisting that this consideration holds for any language-presented phenomenon. But, although this is to some extent true, it is unreasonable to conclude, as Lewis seems to, that interpretation is no more significant in aesthetic than in non-aesthetic experience. Aesthetic perception, more than any other, involves not merely having experiences, but how they are had. When a spectator comes to an aesthetic vehicle, he comes freighted with particular perceptual sets, either momentary or long-standing, that affect his selection from among the elements that constitute the purely sensuous datum. The interpretation is not even temporally separate from the givenness of the datum. How the object is experienced is a function of the spectator's prevailing set: as Wittgenstein emphasizes, he perceives the object as he interprets it.<sup>3</sup> Lewis's contention that the abstraction has the character of esthesis seems to me to be unsound; and since it is cardinal in his account of the aesthetic object, I am constrained to reject the account itself.

Professor C. J. Ducasse advances a view quite unlike any we have considered. He claims that the work of art is identical with the one and only physical product of artistic activity, in short, that the work of art is nothing else than the object that Professors Pepper and Lewis call the aesthetic vehicle.<sup>4</sup> But this view will not do. Consider, for example, the case of a poem. Is the art work

<sup>1</sup> Clarence Irving Lewis, *An Analysis of Knowledge and Valuation* (La Salle, Illinois: The Open Court Publishing Company, 1946), p. 475.

<sup>2</sup> *Ibid.* p. 473.

<sup>3</sup> For a more detailed discussion of this point, see Wittgenstein, *op. cit.* pp. 193 ff.

<sup>4</sup> Curt John Ducasse, *The Philosophy of Art* (New York: The Dial Press, 1929) and *Art, the Critics, and You* (New York: Hafner Publishing Company, 1944).

the sign-design of the printed poem or is it the sign-events that constitute the several occurrences of the sign-design? If the latter, then there are as many works of art associated with a given poem as there are copies of it; if the former, then we are faced with a contradiction. For the sign-design is not a physical object or event; it is a class of printed marks to which belong all sign-events of a particular kind. Since, according to Ducasse, the work of art is a physical object, not an abstraction, he must reject the thesis that the poetical work of art is a sign-design. But the alternative entails that one poem comprehends a host of art works. This contradicts Ducasse's contention that the art work is identical with the one and only physical product of artistic endeavour.

None of these six theories, then, is free from serious defect, none satisfactorily explicates the concept of *the ontological status of the art work*. Perhaps these theories fail to do so because they seek to answer the wrong question. Rather than asking, "What is the ontological status of the work of art?", (as though there might be some sort of existence beyond either physical, sensory or perceptual existence), they might more profitably inquire, "How do the names of particular art works and the designator 'the work of art' and its synonyms function in the concrete contexts of art history, criticism and evaluation?" It may be that the concept of an art work's ontological status has no sharp boundary; that among the various uses for the aforementioned names and designators there is merely the sort of kinship that Wittgenstein calls "a family resemblance". It may be that we actually use names and designators of the sort in question without their having precisely fixed meanings, for the varied application of the concept *work of art* to particular bases permits, so to speak, a whole series of bases upon one type of which we may lean if another should be taken away.<sup>1</sup> But a full discussion of this point is beyond the purpose of this paper, which is merely to demonstrate the inadequacy of an approach of the sort that characterizes the six theories hereinabove rejected.

<sup>1</sup> Wittgenstein, *op. cit.* I 68, 76, and 79; Stephan Korner, *Conceptual Thinking. A Logical Inquiry* (Cambridge, England. The University Press, 1955), especially ch IV.

## VII.—DISCUSSIONS

### THE VERY IDEA OF A SYNTHETIC-APRIORI

THE class of propositions is often trisected as follows: the analytic, the synthetic, and the synthetic-apriori. The status of this third entry is in perennial dispute. Is *any* proposition correctly labeled 'synthetic-apriori'? Or is this very label somehow nonsensical? Perhaps the expression 'P is a synthetic-apriori proposition' makes sense, but is never true since there is no suitable value for the variable P.

These questions require re-orientation. The thesis of what follows is that 'synthetic-apriori' does not label a *type of proposition* at all. Two quite different things are characterized by this designation. Moreover, the *idea* of a proposition which is at once synthetic in structure, yet justified apriori is not inconsistent. It is just that, as a matter of fact, perhaps there are no synthetic-apriori propositions.

Consider the bisection of the class of all propositions along the line 'analytic-synthetic'. P is analytic if, and only if, its negation is of the form (or leads to something of the form)  $Q \sim Q$ . Analytic propositions are thus non-obvious tautologies, their tautological character can be revealed by definitional transformations. However, a synthetic proposition is such that its negation,  $\sim P$ , is not of the form (nor does it entail anything of the form)  $Q \sim Q$ .

This division is exclusive and exhaustive. The negation of *any* proposition must be such that it is either of the form  $Q \sim Q$  (or entails something thus formed),—or its negation is not of this form (nor entails anything thus formed).

It apparently follows from this that analytic propositions cannot but be true. Their negations are self-contradictory, i.e. entail any proposition whatever. And synthetic propositions, since they have equally well-formed and otherwise meaningful negations, must be adjudged true only in virtue of contingent matters of fact.

The knife called 'apriori-aposteriori', however, cuts the class of propositions through quite a different stratum. To characterize a proposition as 'apriori' is to say nothing whatever about its formal structure, or the structure of its negation, or consequences derivable therefrom. It is, rather, to remark the mode whereby the truth of the proposition is discovered. A proposition is apriori if its truth is established without recourse to any possible experience (past, present, or future). A proposition is aposteriori if, in order to justify its truth, reflexion alone is insufficient. Some appeal to some experience is required.

No detailed defence should be required for wishing to distinguish (1) characterizing the *structure* of propositions (and their consequences), from (2) characterizing the *mode of justification* of propositions (and their consequences). One can discuss the analyticity or non-analyticity of P without considering what appeals must be made to establish P's truth. (The third paragraph of this paper does

just this!) The analyticity-non-analyticity question is settled by entertaining  $\sim P$ , tracing its implications, and finding (or not finding) something of the form  $Q \sim Q$ . This kind of enquiry is neutral concerning how  $P$  is established—or even *whether*  $P$  is established.

*Analytic-synthetic*, therefore, divides types of propositional structures. *Apriori-aposteriori*, however, divides types of propositional justifications. These are quite different.

Suppose one identified 'analytic' with 'apriori',—i.e. gave these terms precisely the same meaning. Only then could the designation 'synthetic-apriori' be a contradiction in terms. But, for reasons given above, there are no grounds for this identification. Indeed, it is difficult to see that any judgment concerning the justification of a proposition follows *logically* from a decision as to its analyticity or non-analyticity. The dichotomies are *that* different.

Many levelheaded philosophers have thought peculiar the very idea of synthetic-apriori propositions. In what would this peculiarity consist? The considerations above are calculated to suggest that the peculiarity cannot reside in the fact that this idea is itself inconsistent. How does one *demonstrate* that a proposition whose negation is consistent cannot be justified without recourse to experience? To have learned that a proposition's negation is structured this way rather than that entails nothing about its mode of justification. Not directly. Crudely put, learning that a proposition is justified *aposteriori* is to learn something *in addition* to the fact that its negation is consistent. The idea of a proposition being synthetic does not, by itself, rule out the possibility of its justification *apriori*.

Above all, 'synthetic-apriori' does not designate a new category of *proposition*. Synthetic-apriori propositions (if such there are) are synthetic propositions, propositions whose negations are consistent, and entail nothing inconsistent. This much alone does not mean that there *are* synthetic-apriori propositions. It suggests, however, that the claim that there *couldn't be* synthetic-apriori propositions may be ill-founded, unless based on much broader, and hence more arguable, philosophical considerations. Perhaps there is not one unquestionable candidate for the status 'synthetic-apriori proposition'. But this may just be a matter of fact, and not a consequence of logic.

Let us ask how, in fact, analytic propositions are justified. We know that  $P$  is analytic if and only if its negation is inconsistent (or entails what is inconsistent). Hence, demonstrating that  $P$  is analytic requires doing the same things as to show that  $P$  is forever true. Since its negation is self-contradictory, it could not but be true—a fact we learn from logical manipulation, not from experience. Even so, since the consequences of this distinction will bear down heavily in the case of synthetic propositions, I must insist that *demonstrating  $P$ 's analyticity is not the same as establishing that  $P$  could not be false*. The operations of both procedures may be identical—but the ends are different.

To say that a proposition is analytic is *not* to say just that it is

forever and always true That  $P$  is forever true follows from (but is not identical with) the fact that  $P$ 's negation is self-contradictory

Confusions result from failing to make this distinction. Thus, when analytic propositions are said to be those which are forever true and couldn't be false, the idea of analyticity collapses into that of invulnerability. Thus the class of analytic propositions becomes populated with all sorts of propositions which, although felt to be certainly true, lack self-contradictory negations. Certain physical principles, religious utterances, and even some moral precepts have been characterized as analytic solely on the grounds that they are invulnerable to disconfirmation But this locates an accidental feature of analyticity as its defining characteristic. *Of course* analytically true propositions are invulnerable to disconfirmation This is *because* their negations are self-contradictory So questions about whether  $P$  is analytic remain distinct from questions concerning how  $P$  is established. I prove the former by revealing inconsistencies in  $\sim P$ , or in its consequences The latter issue involves arguing from the fact that  $\sim P$  (or one of its consequences) is inconsistent to the conclusion that therefore  $P$  must be forever true The second undertaking is different from, indeed presupposes, the first

This distinction becomes critical with synthetic propositions, and for the point of this paper. When I have established that a proposition is synthetic I have *only* established that its negation is consistent. This is done by almost purely logical means. Justifying the truth of the proposition is quite different in this case For, a proposition may be synthetic whether true or false  $P$  is synthetic if  $\sim P$  is consistent as well as  $P$ , of course One must add this to block calling a contradiction, whose negation is consistent, 'synthetic'. Establishing  $P$ 's truth requires something else to be done. What?

It remains to be shown to me that *what* else must be done to establish  $P$ 's truth can be read off simply and directly from the fact that  $\sim P$  is consistent But what is logically wrong with the *idea* that the class of non-contradictory propositions having consistent negations (i.e. synthetic propositions) divides into those whose truth is justified without recourse to experience (i.e. *a priori*), and those whose truth is justified only by recourse to experience (i.e. *a posteriori*)? To rule out the former possibility is simply to shout (without giving reasons) one of the central dogmas of empiricism, which latter, I need hardly remark, is *not* a set of deductive principles But since this is just what is at issue (i.e. does 'synthetic-a priori proposition' make sense), the matter thus put need not be pursued.

I have no clear notion of what it would be like to justify by reflection alone the truth of  $P$  when  $\sim P$  is consistent But this *may* be only a fact about *me* I cannot see how of two equally consistent alternative propositions ( $P$  and  $\sim P$ ), reflection alone will determine which describes the facts But to say this does not *prove* that there cannot be synthetic-a priori propositions I have but softly expressed an article of the empiricist's faith. to wit, that as a working hypothesis, it seems unlikely that any genuine candidates for synthetic-a priori will be forthcoming. This conjecture is not



justifiable by logic alone. And logic alone cannot demolish a non-empiricists' position.

This discussion must not be allowed to involve a host of propositions which are at once synthetic (*i.e.* having consistent negations) and are yet unconceivably false. It is perhaps invulnerably true that I cannot be in two places at once, that I am now conscious and typing out these words, that there cannot be a perpetual motion machine, etc. But while these are both invulnerably true and also synthetic in form, such propositions will not serve as candidates for synthetic apriority. Their invulnerability results not so much from a direct, non-experimental demonstration of their truth, but rather from an implicit, yet far-flung reference to entire *systems* of empirical knowledge whose very pattern depends upon assuming the truth of such statements. The invulnerability of a 'genuine synthetic-apriori statement' cannot consist simply in pointing out the disastrous systematic consequences of entertaining the negation to such a proposition. The credentials of such a spectacular propositional entity must be set out directly by reflection on the assertions made by  $P$  and by  $\sim P$ .

Of propositions which are systematically invulnerable-yet-synthetic, there are many examples at the philosopher's tongue-tip. Of propositions whose negations are consistent, but yet whose truth will be apparent to anyone who understands them—I cannot think of one single example. But this implies nothing of logical importance about the *idea* of a synthetic-apriori proposition. No mere fact about what I, or anyone, can or cannot do would have such an implication.

The situation, then, is this · that of all known non-contradictory propositions whose negations are consistent, it is factually true that they are established by recourse to experience. In other words, that they are contingent is (in a sense) a factual truth about such propositions. That they are synthetic is a necessary truth. That certain claims are formally structured such that their negations are inconsistent is a claim which, if true at all, could not but be true. But that their *justifications* are contingent upon experience is a claim of a different kind. This is *just* a matter of fact. It may be said to describe only the 'explored regions' of the modes of justification for synthetic propositions. In itself it contains no argument against a proposition with a consistent negation being justified in some different, possibly non-experiential manner. We *do* know what it is like for some propositions to be justified without recourse to experience. With analytic propositions we know why such recourse is needless. We are not in the same position concerning synthetic propositions. We may not in fact, know how a proposition with a consistent negation could be justified without recourse to experience. But, short of reiterating the empiricist's manifesto (again without logically binding reasons), I know of no strict argument for ruling out the very idea of a synthetic-apriori

## EXISTENTIAL QUANTIFICATION AND THE "REGIMENTATION" OF ORDINARY LANGUAGE

By an existential quantifier one means an expression of such and such a kind *within some language-system*  $L$ . Strictly then we should speak only of an existential quantifier of  $L$ . The clause 'of  $L$ ' is often omitted but only for ellipsis. Strictly, for explicitness and clarity, it should always be present.

Usually the existential quantifier of  $L$  is symbolized by ' $(\exists x)$ ' or ' $(\exists x)$ ' (or in some other suitable way) where ' $x$ ' is a variable of  $L$ . The variables of  $L$  (supposing for the moment that they are all of one kind) are to be thought of as *ranging* over a certain *domain of individuals*, say  $D$ , as *values*.  $L$  is here a system with a specified interpretation. Part of this interpretation is given by the so-called *semantical rules of range*. For the present we need only one rule of range specifying the domain  $D$ . In fact the rule of range here may be regarded simply as a definition of the phrase 'class (or virtual class) of values of variables for  $L$ '. We let

'ValVbl $L$ ' abbreviate ' $D$ ',

so that the class of values for variables of  $L$ , ValVbl $L$ , is by definition simply the class  $D$ .<sup>1</sup>

In the usual way of formulating (classical) quantification theory in  $L$ , the domain  $D$  is presumed to contain at least one individual. The universal quantifier of  $L$  is usually read with the help of the word 'all' ' $(x)$ ', for example, is then read 'for all  $x$ ' or 'for every  $x$ ' or 'every  $x$  is such that' *where in each case  $x$  is to be understood as an individual in the domain  $D$* . Better readings are therefore 'for all  $x$  in  $D$ ' or 'every  $x$  in  $D$  is such that'. These readings seem clear and unambiguous. If the phrase 'in  $D$ ' is omitted this again is mere ellipsis and strictly it should always be regarded as present. It is not therewith claimed that the various uses of the English words 'all' and 'every' are unambiguous or without need of careful analysis, but only that the contexts 'for all  $x$  in  $D$ ' or 'for every  $x$  in  $D$ ' are as "clear" presumably as any concepts of mathematics or exact science ever are, provided  $D$  is itself a clear-cut domain of objects.

If the foregoing explanations are acceptable (and indeed they are based on more or less standard semantics), the role of the existential quantifier may be clarified as follows. We let

' $(\exists x)$ ' abbreviate ' $\sim (x) \sim$ ',

where ' $\sim$ ' is the sign of (classical) negation. Now ' $\sim$ ' is usually read 'it is not the case that' or simply 'not'. Again there seems to be no essential difficulty in this reading. The proper unabbreviated reading of the existential quantifier ' $(\exists x)$ ' is then 'it is not the case

<sup>1</sup> Of the author's *Truth and Denotation, A Study in Semantical Theory* (Routledge and Kegan Paul, London, University of Chicago Press, Chicago, and University of Toronto Press, Toronto, 1958), pp 160 f.

that every  $x$  in  $D$  is not such that ' or 'it is not the case that for every  $x$  in  $D$  it is not the case that '. Clearly these two phrases may be given the abbreviated reading 'there is at least one individual  $x$  in  $D$  such that ' or '  $D$  contains at least one individual  $x$  such that ' (assuming, as already mentioned, that  $D$  is non-null).

In these readings of '(Ex)' the word 'exists' does not occur. Concoervably '(Ex)' could be read 'there *exists* at least one  $x$  in  $D$  such that ' but all that should then be meant by this phrase is covered by the foregoing explanations. The use of 'exists' here is a mere convenience, a *manière de parler*, a mere abbreviatory reading. There is *some* connection of course with the "ordinary" meaning of 'exists', but not enough perhaps to justify calling the quantifier '(Ex)' an *existential* quantifier. Perhaps it would be better to call it merely an *E-quantifier* or give it some other innocuous label, as Wilfred Sellars has recently suggested.<sup>1</sup>

So far as formal logic and semantics are concerned,  $D$  may be taken as any non-null domain of objects whatsoever, depending upon the subject-matter being formalized in  $L$ . In practice, however,  $D$  is usually a well-defined totality of objects, such as the totality of natural numbers, of real numbers, or of sets in the sense of a mathematical theory of sets, etc. The philosophical constructionalist is free to choose  $D$  *ad libitum*, provided only that it constitute a well-defined totality. In order to have such a totality some clear-cut way of differentiating among the individuals of  $D$  must be given, more specifically, a clear-cut condition under which two distinct objects of  $D$  differ from each other.

In his *Word and Object*<sup>2</sup> Quine states that "(o)n the whole the canonical systems of logical notation [including quantifiers] are best seen not as complete notations for discourse on special subjects, but as partial notations for discourse on all subjects". This passage and its embellishment are rather central to Quine's philosophy of logic. "Practical temporary departures from ordinary languages", he notes, are to be tolerated on occasion short of using the "canonical notation" of symbolic logic. Certain further departures also may serve the purpose of simplification of theory. The result may no longer be an ordinary language but a "semi-ordinary" one, augmented with the full symbolism of modern logic.

The quoted passage from Quine is not free from ambiguity. But presumably it is intended to say or at least to entail that variables are "best" construed as ranging over *all objects* and hence that the universal quantifier is "best" read 'for all objects  $x$ '. No reference to the domain  $D$  is needed or called for, nor need any reference to a language-system  $L$  be made.

It was urged above that the phrase 'for all  $x$  in  $D$ ' is clear and

<sup>1</sup> See his "Grammar and Existence: A Preface to Ontology", *Mind* LXIX (no 276, 1960), 499-533, esp the footnote, p 508.

<sup>2</sup> (The Technology Press of the Massachusetts Institute of Technology and John Wiley and Sons, New York and London, 1960), p 160.

unambiguous, provided  $D$  itself is a clear-cut domain of objects. What meaning now are we to attach to Quine's idiom 'for all  $x$ ' without specification of  $D$  as a reading of the universal quantifier? Perhaps the idiom 'for all  $x$ ' without specification of  $D$  is meaningful in ordinary language, but this is doubtful. It is not clear that ordinary language contains variables and quantifiers at all. Quine as much as admits this latter point and therefore speaks occasionally of "semi-ordinary" language. But however this may be, the modern logic of quantification requires fundamentally the specification of a domain  $D$ . No domain  $D$ , no quantifiers. In fact it is simply meaningless, according to modern semantical theory, to use the quantifiers (interpreted in the intended way) without specifying a domain  $D$  as the range of the variables.

Very well, then, Quine might answer, take  $D$  as consisting of *all objects*. The question then arises as to what meaning can be given to the phrase 'all objects'.

Quine of course prefers physical objects to others. He works hard to defend and justify this preference, however, but he does not altogether succeed in giving the phrase 'physical object' a clear meaning. Nonetheless one would not cavil over taking  $D$  as the domain of *physical* objects. Set theorists demand another domain for their variables to range over, a domain of *sets* or *classes*. But the theologian needs another, and the literary critic still another, and so on.

Quine would no doubt allow to the set theorist his domain of sets. But within a language for set theory the quantifier ' $(x)$ ' should be read 'for all *sets*  $x$ ' (assuming for the moment that the theory contains no non-sets). Nothing whatsoever is gained by insisting upon the reading 'for all *objects*  $x$ '. In fact, quite the contrary, a great deal is lost. What are we to include as objects? Angels, dreams, dispositions, works of art, values, etc.? Russell's class of all classes which are not members of themselves? Sentences or inscriptions of such which say of themselves that they are not true? Clearly there must be *some* restrictions here to avoid inconsistency, on the one hand, and to gain a pittance of clarity on the other. Hence the phrase 'for all objects  $x$ ' really cannot mean at all what Quine apparently wishes it to. Suitable limitations must be imposed by restricting the variables to range over some fixed domain  $D$ .

We see then that Quine's reading of the universal quantifier ' $(x)$ ' as 'for all objects  $x$ ' is not the "best" one; it is not a "clear" one, nor in the strict sense is it even a meaningful one. Similar comments apply to his reading of the existential quantifier ' $(\exists x)$ ' as 'there exists at least one object  $x$  such that'. All that can significantly be meant by the latter, in some  $L$ , is that within the domain  $D$  of  $L$  there is at least one object  $x$  such that

If  $D$  is a domain of physical objects (in some sense), then and only then is there connection between the existential quantifiers of  $L$

and physical existence (in the sense in which this may be spoken of in *L*). It is simply an error to suppose that this connection rests merely upon the existential quantifier without specification of *D* and of *L*.

The relation between the idioms 'for all objects' and 'for some object' or 'there exists at least one object such that' in ordinary language and the quantifiers is much more distant than Quine thinks. In fact, strictly speaking, there is no connection at all, for we simply have no quantifiers (with the intended interpretation) until a language-system *L* with a domain *D* is specified. Once it is specified, we may then study the interconnections between *L* and the relevant "parts" of the ordinary language. But this is a very complex type of interrelationship, which Quine's comments in no way serve to illumine. Similar remarks, incidentally, apply to the interconnections between the truth-functional connectives and the "ordinary" words 'and', 'or', etc. Logical analysts often speak as though there is a close connection here, but precisely what this connection is is far from clear. The enormous complexity in the use and usage of words and phrases in ordinary language, even of such simple words as 'and' and 'or', militates against their easy assimilation to a too clear-cut and indeed artificial "regimentation".

That Quine's reading of the quantifier '(*x*)' as 'for all objects *x*' is unsound may be seen also by noting the way in which the quantifiers of *L* are given an interpretation within a systematic semantics. This is accomplished by means of an *adequately defined truth-concept*. In particular it must obtain within the meta-language that '(*x*)—*x*—', where '—*x*—' is a sentential function of the one variable '*x*', is *true in L* if and only if for all objects *x* in *D* (of *L*), —*x*—. We are assured that the universal quantifier behaves in its proper logical way by this principle of semantics. In fact the *only* method we have of assuring this, *i.e.* of giving the universal quantifier its proper interpretation, is by means of such a principle. Perhaps Quine has in mind to achieve this in some other way, but if so he does not tell us how. In particular Quine cannot take *L* as ordinary language without presupposing an adequate truth-concept for ordinary language. Against this presupposition Tarski has argued forcibly in § 1 of his *Der Wahrheitsbegriff*.<sup>1</sup>

Carnap's distinction between questions internal and external to a framework or language-system is useful here.<sup>2</sup> Prior to or independent of the selection of a language-system *L* with a domain *D* of objects as its fundamental domain, the quantifiers are without meaning. They are given meaning only with the formulation of *L*, *i.e.* by the explicit listing of the syntactical and semantical rules

<sup>1</sup> In *Logic, Semantics, Metamathematics* (Clarendon Press, Oxford, 1956), pp. 152-278.

<sup>2</sup> See his "Empiricism, Semantics, and Ontology", in *Meaning and Necessity*, 2nd edn. (University of Chicago Press, Chicago, 1956), pp. 205-221.

determinative of *L*. But Quine apparently wishes to give them meaning prior to and indeed independent of such formulation. He mistakes, in other words, an internal matter for an external one. The situation is much as though Quine would wish to give the integral sign ' $\int$ ' significance independent of the system of real numbers or the ' $\epsilon$ ' of membership significance independent of some specific axiom-system for set theory. The quantifiers are like ' $\int$ ' and ' $\epsilon$ ' in having significance only within well-specified language-systems. The meanings in ordinary language which they seem to have are usually rather inexact and either reflect the more technical meanings of some years back or suggest more technical meanings yet to be given.

The points raised above seem rather crucial to Quine's view as to the way in which logic serves as an instrument of "regimentation" for ordinary language. In fact if the foregoing argument is sound, not very much of Quine's view as to the interconnection between logic and ordinary language remains. His view seems too simple and premature. An adequate characterization of this interconnection must await much careful work on the part of linguists, logicians, and philosophic analysts. But this will not be easy and we must not gloss over difficulties by refusing the help and clarification which modern semantics can give.

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## ON THE ALLEGED OBJECTIVITY OF MORAL JUDGMENTS

THERE is an oft-quoted passage of Bertrand Russell's in which he expresses his dissatisfaction with the subjectivist approach in ethics. Having argued that moral judgments simply express desires which one feels or wishes others to feel, he goes on

But what are "good" desires? Are they anything more than desires that you share? Certainly there *seems* to be something more. Suppose, for example, that some one were to advocate the introduction of bull-fighting in this country. In opposing the proposal, I should *feel*, not only that I was expressing my desires, but that my desires in the matter are *right*, whatever that may mean. As a matter of argument, I can, I think, show that I am not guilty of any logical inconsistency in holding to the above (so subjectivist) interpretation of ethics and at the same time expressing strong ethical preferences. But in feeling I am not satisfied. I can only say, while my own opinions as to ethics do not satisfy me, other people's satisfy me still less. ("Reply to Criticisms", in *The Philosophy of Bertrand Russell* ed. Schilpp, p. 724.)

Most recent writers in ethics, whilst sharing in general Russell's subjectivist approach, do not share his dissatisfaction with it. This is not because they are simply tougher-minded subjectivists than he and so able to hold to their doctrine without experiencing deviationist doubts, what some, at least, of them claim, in effect, is that they are subtler-minded subjectivists and so able to show that a moral judgment is, in fact, what Russell would have liked to think it was, namely "something more" than an expression of feeling or taste.

What I wish to point out is that two lines of argument, which have recently been taken to establish the objectivity of moral judgments, do so only in an unusual, restricted sense of that word, and it is misleading to suggest otherwise.

### I

One of these lines of argument concerns the *reasons* which are given for moral judgments. Professor Paul Edwards, with explicit reference to the passage from Russell quoted above, states it thus:

My theory or Russell's own theory, supplemented by a consideration of the *reasons* for moral judgments, easily clears up the source of this dissatisfaction (so Russell's) . . . "The introduction of bull-fighting in the United States would be a bad thing", in addition to expressing something concerning the speaker, makes some such objective claim as, "The introduction of bull-fighting would lead to avoidable pain for innocent animals. . . ." Russell's desire is objectively superior in the sense that its satisfaction would prevent the suffering of innocent animals . . . etc. The satisfaction of his opponent's desire would have altogether different consequences. This is, I think, what Russell means by "superior" in the sense of referent. It is certainly the sort of thing that I would mean. If the facts concerning bull-fighting are as I described them a moment ago it is clear that Russell

is right To the extent to which an advocate of bull-fighting means the same by "superior" he would be mistaken If he means something different, then he may or may not be mistaken, but Russell's claim remains objective and true (*The Logic of Moral Discourse*, p 214)

The other line of argument concerns the *universalsability* of moral judgments Mr Bernard Mayo says that, faced with the choice of calling moral judgments subjective or objective, he would choose the latter (*Ethics and the Moral Life*, p 45) He is "against subjectivism" (*ibid.*). Moral judgments, in his view, are not quite statements of fact, but they are much more like statements of fact than like expressions of feeling or taste (*op. cit* chap v) Mayo finds the ground for this in the required universalsability of moral judgments, which he states thus

A moral judgment must be universalisable, firstly, in the sense that it applies not to a particular action, but to a class of actions . . . Secondly, . . in the sense that it applies . . to everybody . . And . . thirdly . . in the sense that others besides the speaker are assumed to share it (*op cit* p 91)

If Edwards and Mayo claimed simply to be explicating some of the conventions in accordance with which moral discourse proceeds, one could have no quarrel with them But they claim more than this Reason-giving and universalisability are taken to constitute a radical difference between moral judgments and expressions of feeling or taste, and, in Edwards at any rate, it is claimed that the ground for dissatisfaction with subjectivism, such as Russell felt, has been removed.

## II

Factual reasons, however, can be given, not only for our moral judgments, but for our likes and dislikes also.  
Compare

"Strawberries are nice."

"Why?"

"Because they are sweet"

with

"Bull-fighting is wrong."

"Why?"

"Because it causes avoidable pain."

The reason-giving sentence, in each case, is factually true Now, Edwards says "If the facts concerning bull-fighting are as I described them . it is clear that Russell is right . . Russell's claim remains objective and true" (*op cit* p 214) His point appears to be that the truth of the factual reasons which could be given for Russell's judgment confers upon it an objectivity which does not belong to expressions of feeling or taste But, if the factual truth of "Bull-fighting causes pain" confers objectivity on the judgment, "Bull-fighting is wrong", then the factual truth of



"Strawberries are sweet" must confer an exactly similar objectivity on the expression of taste, "Strawberries are nice".

Now consider universalisability. To say that "Bull-fighting is wrong" is universalisable is to say that there is a universal moral principle which, together with a true statement about certain of the non-moral characteristics of bull-fighting, entails this judgment. The non-moral characteristics may be explicated one by one in discussion thus

- A "Bull-fighting is wrong"
- B "Why do you say that?"
- A "Because it causes pain"
- B "So you think acts which cause pain are wrong?"
- A "Yes"
- B "But you fought in the war. Why weren't you a conscientious objector?"
- A "That's different. I should have said that I think acts which cause pain that is avoidable are wrong"
- B "But surely the war was avoidable! The Allies could have given in to Hitler. Do you think they ought to have done so?"
- A "No. What I should have said is that acts, which cause pain that is avoidable without great harm resulting, are wrong. It would have done great harm to let Hitler have his way."

A's universal moral principle at last becomes clear: it is that all acts, which cause pain and are unavoidable and, if left undone, would not result in great harm, are wrong. Notice what B is really pressing A to do in the above conversation: he is pressing him all the time to say *more precisely* what it is that he is morally for or against.

Now, it is surely possible to conceive of someone being pressed to say more precisely what he is for or against, not on a moral issue, but in a matter of taste.

- C "Strawberries are nice."
- D "Why do you say that?"
- C "Because they are sweet."
- D "So you consider sweet things nice?"
- C "Yes"
- D "But I just saw you refuse a humbug with a grimace. Surely you like humbugs, if you consider sweet things nice!"
- C "No. What I should have said is that I consider things which are sweet and succulent to be nice"
- D "Then you like grapes?"
- C "No. What I should have said is that I consider things which are sweet, succulent, and red in colour to be nice"

This conversation is eccentric but not inconceivable. If this way of talking gives A's judgment about bull-fighting objectivity in the former conversation, it must do the same for C's remark, "Strawberries are nice" in the latter conversation.

The differences in the way we talk about moral issues and the way

we talk about matters of taste are empirical. When someone says, "Strawberries are nice", we do not normally press him for his reasons or insist that he show us how the niceness of strawberries is an instance of some more general niceness. It is not logically impossible to do so, it is just that we do not

### III

Why, then, are there these conventions of reason-giving and universalsability so far as moral judgments are concerned? The answer lies in the fact that these conventions make it, on the one hand, *possible always to open* an argument, when someone has delivered a moral judgment, and, on the other hand, *impossible to refuse to join* in argument, once one has delivered a moral judgment oneself

In this connection, Mr. Jonathan Bennett recently made an important point about the universalsability principle. He said that it should be reformulated thus: a judgment is a moral judgment only if the person who makes it accepts some universal moral principle which, together with a true statement about *some but not all* of the non-moral characteristics of the act or state of affairs being judged, entails this judgment. If we read "*all*", instead of "*some but not all*", then the judgment will apply only to this particular act or state of affairs, and it will be just a logical trick to say that it passes the universalsability test. In favour of his more careful formulation, "*some but not all*", Bennett points out that a moral judgment on X will then not apply only to X but to other acts or states, which have certain non-moral characteristics in common with X, and consequently "there can be moral argument" ("Moral Argument", *MIND*, vol. lxxix (1960)). Where A has judged X to be right and stated the non-moral characteristics of X because of which he thinks it right, it is open to B to adduce some counter-instance, Y, which has the relevant non-moral characteristics in common with X, but which A does not judge to be right. We saw this happening in the above conversation about bull-fighting. As long as the list of non-moral characteristics is not so complete that it will fit only X and nothing else, the possibility of the counter-instance, and so of continued moral argument, remains open.

Matters which we discuss in the universe of moral discourse are matters about which we want to be able to argue. Hence the rules which make this possible. We have strong desires here and want others to have them too. To make bull-fighting a subject of moral discourse is to put oneself, and anyone who is prepared to discuss it with one in these terms, in a position where a decision has to be made, where this decision can be argued about, and where others can be persuaded to make it also. This is certainly to differentiate it from matters of feeling or taste in general. But does this make it another kind of thing altogether, or simply a member of a sub-class of matters

of feeling or taste, to which certain rules apply? It is significant that many moral issues were originally matters of taste in the accepted sense of that phrase. For example, there was a time when it would have been considered a matter of personal inclination whether or not one kept a slave, but a change in sentiment concerning their fellow-men who were slaves made some people begin to ask, "But is it *right* to keep a slave?" They had a strong desire to stop slavery and so they took the subject over into the universe of moral discourse and began to talk about it in terms of right and wrong. They passed verdicts and invited others to do so, gave and required reasons for these, and, in short, began to argue and persuade in all the ways which moral discourse makes possible.

#### IV

If the alleged objectivity of moral judgments is taken to mean simply that, once we have begun to talk about some matter of taste in moral terms, we cannot say anything significant about it unless we keep the rules of moral discourse, then this is indisputable. But this is not to abandon the subjectivist position in ethics. It goes no way at all towards what has traditionally been meant by the objectivity of moral judgments, namely, that they are statements of fact known by a faculty of moral cognition. It is cold comfort to the philosopher or plain man, dissatisfied, as Russell was dissatisfied, with subjectivism, to tell him that, though he is merely expressing his own feelings, he is doing so in accordance with certain linguistic conventions. It may be that there is no comfort for him. It may be that we should urge him to reconsider whether it is not enough that these matters which we call moral issues are ones about which he and others feel passionately. But this much is certain: if the objectivity which he would like to think his moral judgments have is to be established, it will have to be on grounds other than those which we have been considering.

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## VINDICATION OF ETHICAL INTUITIONISM

ACCORDING to Professor Nowell-Smith's critique of Intuitionism (*Ethics*, chap 3) the Intuitionists cannot from a direct intuition of moral facts derive the notion of actually doing one's duty. I might know that X is my duty as a piece of theoretical knowledge, just as I would know any other scientific or theoretical fact; but the knowledge by itself cannot tell me why I should do X. From the knowledge that X is my duty, I cannot derive the 'ought', namely, that I ought to do X. In this, Nowell-Smith asserts, there is as much a 'gulf' or 'gap' in the Intuitionist view-point, as in that of the teleological moralists.

In this essay I shall try and defend the Intuitionist position against this critique, in the main, by showing that there is no 'gap' or 'gulf' involved in an ethics based upon Intuitionism. In the first place I would like to introduce a distinction, which I consider to be of great significance, as much of the plausibility of Nowell-Smith's critique is due to the slurring over of this distinction. The distinction is indicated by the recognition that the 'gap' may be shown to occur at two levels of moral discourse, these I call the ethical level and the meta-ethical level. At the ethical level we are concerned with particular moral acts in concrete moral situations involving some dilemma or perplexity. The 'gap' is expressed in the question I see X is good, or, X is my duty, but, why should I do X? In the words of Nowell-Smith: "But if 'X is right' and 'X is obligatory' are construed as statements to the effect that X has the non-natural characteristic of rightness or obligatoriness which we just 'see' to be present, it would seem that we can no more deduce 'I ought to do X' from these premises than we could deduce it from 'X is pleasant' or 'X is in accordance with God's will'. A gap of which ordinary language knows nothing has been created between 'X is obligatory on me' and 'I ought to do it'; and this gap requires to be bridged" (p. 42). At the meta-ethical level the 'gap' is expressed in the general question: "Some things, I have now learnt, are right and others wrong; but why should I do what is right and eschew what is wrong?" (p. 41). Nowell-Smith himself does not seem to be alive to the distinction between the two orders, although in different places he frames the two questions separately.

In the light of this distinction I shall be concerned with making the following two points: (a) For Intuitionists (as well as Teleologists) the more general question, expressing the 'gap' at the meta-ethical level, is irrelevant, if not entirely meaningless. (b) At the ethical level the 'gap' or 'gulf' does exist for the Teleologists, but not for the Intuitionists.

Of the two questions, at the ethical and at the meta-ethical level, both Intuitionists and Teleologists are logically required to answer only the ethical question. And the worth of neither is impaired

solely by the inability to answer the meta-ethical question. This will become evident if we begin with a clear realization that Intuitionism is an ethical theory (or, more precisely, a theory *within* ethics) about methods for determining what things are good, or what actions are obligatory. It is one theory (within ethics) about methods among other theories. The Teleological theory (be it of the metaphysical, theological or naturalistic species) about methods or criteria is another competing theory *within* ethics. As such, it is beyond the scope and province of either theory to attempt answers to meta-ethical questions like 'Why ought one to be moral?' 'Why ought one to do one's duty?' Or : 'Why ought one to do the good?' In a very important sense Ethics may be said to be the science which is concerned with the analysis and interpretation of the Good and/or Right; hence, within Ethics itself the above questions lose their motive. Good and/or Right are the basic concepts which underlie the moral activity; they are the foundations upon which moral theories are founded. The function of moral theories is to help circumscribe Good and Evil. But, both are within the moral purview. Hence, the real antithesis is not between moral and immoral, but between moral and non-moral. And, even if nihilism is a 'live' alternative, neither Intuitionism nor Teleology can help decide the issue—the answer remains outside the function of both these ethical theories.

But *within* Ethics itself it is perfectly meaningful and legitimate to compare the two rival theories in order to decide the superiority of the one or the other. It is at the ethical level—the order of ethical discourse—Intuitionists challenge the claim of the Teleological moralists to have supplied better methods for the determination of the Good or the Right. The challenge rests on a point of logic shown by Hume and elaborated by Moore as the 'naturalistic fallacy'. Moral discourse (if genuine at all) is autonomous, any attempt to derive ethical categories from non-ethical categories involves a gulf which cannot be logically bridged. But, according to Intuitionism there is no logical gulf involved in its own theory.

In a moral context, at the ethical level, when a person asks, 'What ought I to do?' he means to ask 'What is morally the right thing to do under the circumstances?' 'What is my duty in this situation?' 'What is the good in this case?' Hence, the general assumptions underlying the question are (a) the questioner knows, and does not doubt, that what he must do is the good or right (b) But, he is perplexed about what the good or right is. In a concrete moral situation, therefore, the questioner seeks *knowledge* of what the good or right is. Of course, the question might be addressed to another, or to himself by the questioner; important is the fact of perplexity, perplexity, not whether he should do good and eschew evil, but what is the good or right in the circumstances which he must follow.

I will, now, first show that if this *knowledge* is secured through the teleological method, there will remain a logical gap in the theory,

a gap which can only be bridged through an intuition, proving the autonomy of morals. Then, I will go on to show that not only is Intuitionism the only logically satisfying theory, but that the relevant knowledge supplied through the method of intuitionism does not leave us with any logical gaps in the theory.

### *The Teleological Analysis*

To the questioner who asked 'What ought I to do?', the Teleologist might answer. 'You ought to do X.' (moral judgement). Questioner 'Why?' Teleologist. 'X increases human happiness.' We see in this instances how there still remains a gap which frustrates the questioner's inquiry. By his query, 'What ought I to do?', he wanted to know. What is the good/right/duty in this situation? But, the information he has obtained is about an act X which increases human happiness. His inquiry can only be satisfied if he can be shown that acts which increase human happiness are good/right/duties—that there is no gap between acts which increase human happiness and goodness/rightness/obligations. But, as soon as the Teleologist attempts to bridge the gap by advancing the required premise (that: All acts which increase human happiness are good/right/duties), the premise itself being a moral judgement, the Teleological moralist is required either to explain further on *teleological grounds* the new moral judgement, in which case the explanation is merely pushed back one step, or, he must admit the intuitional basis of his premise. Ethical categories, therefore are shown not to be derivatives and cannot be explained by pure teleology. In other words, as Moore did such a lot to clarify, moral categories cannot be reduced to non-moral categories. Moral discourse is autonomous.

Nowell-Smith himself agrees that "the strength of Intuitionism lies in its uncompromising insistence of the autonomy of morals. Ethical sentences are not, as Moore clearly shows, psychological or metaphysical or theological sentences." Of course, both the Teleologist and the Intuitionist might be mistaken about the objectivity of the moral consciousness, since both presuppose the answering of meta-ethical questions. Both theories operate upon the basis of a metaphysical foundation. But, at the ethical level, Intuitionism, in spite of its mystical flavour, is more satisfying, logically, than Teleology. The validity of the Intuitionist polemic against Teleology follows from the ultimate recognition that Teleology by itself, without recourse to a single intuition, is impotent to explain the moral consciousness.

### *The Intuitionist Analysis*

Intuitionism involves no theoretical gap. To the questioner who asked, 'What ought I to do?' the intuitionist answer would be: 'You ought to do X'. Questioner. 'Why?' Intuitionist. 'X is good/right/duty'. There is no theoretical gap which frustrates the

questioner's inquiry. By his question, 'What ought I to do?' he wanted to know 'What is the good/right/duty in this situation or context'. Once he 'sees' for himself that X is good/right/duty, the further question, 'Why ought I to do X?' becomes meaningless in the context. This question, it will be remembered, is the question at the ethical level according to our distinction. This question becomes meaningless as the point behind the question was that the questioner, as a person in moral perplexity, sought some definite *knowledge* as to *what* the right or good in the particular case was—and he has been supplied with the knowledge. There is no inference and there is no gap as the knowledge supplied is moral knowledge, or moral fact, not non-moral fact, as is the case with the Teleological answer. The non-moral content of an answer to a moral question would leave a gap and require an inference.

Intuitionism, therefore, at the ethical level, involves no gap or gulf, and the question, 'I know X is right, but why ought I to do X?' is not so much "wicked, immoral, sinful, inhuman", reactions anticipated by Nowell-Smith, as it is meaningless. Nowell-Smith's error lies, it seems to me, firstly, in overlooking the distinction I have introduced in this article, secondly, in a false psychology. He writes "A new world is revealed for our inspection, it contains such and such objects, phenomena, and characteristics, it is mapped and described in elaborate detail. No doubt it is all very interesting. If I happen to have a thirst for knowledge, I shall read on to satisfy my curiosity, much as I should read about new discoveries in astronomy or geography. Learning about 'values' or 'duties' might well be as exciting as learning about spiral nebulae or water-spouts. But what if I am not interested? Why should I *do* anything about these newly-revealed objects?' (p. 41). But I suggest that this whole passage is based upon a totally false psychology of the moral situation, and on a misconception about what the appropriate emotions are in the context. At the ethical level, when engaged in a situation involving moral choice, one does not view the moral world with a detached 'interest', 'wonder', 'curiosity', or 'excitement'. The appropriate emotion is one of anxiety, which ranges from perplexity and uneasiness to sheer moral agony. Hence at the ethical level of a man engaged in a concrete moral situation, the question is not only meaningless, but absurd.

If it is argued that the point behind the question 'I know X is right, but why ought I to do X?' is the more general one of 'Why should *anyone* do *anything* which he knows to be good or right?' Or, 'Why should one be moral?'—this shifts the problem to the meta-ethical level. As already indicated, it is not the burden of Intuitionism to answer this question, as the latter lies outside its scope. Considering the function of Intuitionism as a method, it is a wrong kind of question to feed to the method, and expect an answer.

## AN EXEGETICAL POINT IN ARISTOTLE'S NICOMACHEAN ETHICS

WHAT does "according to the *orthos logos*" mean in Aristotle's *Nicomachean Ethics*? Ross translates: "according to the right rule". Now a rule is something general, something of the form: "In circumstances C. do A." But if *phronêsis* or "practical wisdom" (which involves a grasp of the *orthos logos*) gave us rules, it would be reasonable to suppose that Aristotle would give us some examples of them. But surely he does not. Therefore there is a presumption that the question: "What does it mean to act according to the *orthos logos*?" cannot be answered by giving examples of rules action according to which would be such action.

Now *phronêsis* is handled in the Sixth Book as if it were (or were closely allied to) the ability to recognize things in general and goods-for-man in particular. *Phronêsis*, or something very like it, is bound up with the ability to recognise chicken (1141 b 19) or stagnant water (1142 a 23). Therefore one is tempted to say that the *phronimos* or "practically wise" man is the man who can recognise a good-for-man when he meets one, the man who, in a situation in which it is better for man as a social creature to do A rather than B (because X, which A will further, is a good, whereas Y, which B will further, is not) recognises that this is the case, and, pursuing the good which he has recognised, acts accordingly. This suggests an interpretation of "according to the *orthos logos*" as "according to a correct appreciation of the situation". On this interpretation the *orthos logos* is always particular. The *orthos logos* of this situation is that it would be a pity if Y were allowed to happen (and it would happen if B were done), whereas it would be a good thing if X were brought about (as it would be if A were done). Being in this way particular, Aristotle cannot (logically) give examples of "what the *orthos logos* says" for the *orthos logos* is not a fount of generally applicable principles. Every situation has its *orthos logos*, which is the correct account of how the goods-for-man are disposed in that particular situation. The point of the doctrine of "practical wisdom" is that you cannot usefully generalise about how goods-for-man are disposed in S-type situations, since the recognition of goods-for-man as they occur in concrete situations is a matter of trained discernment (such as young men cannot practise, etc.). I can only teach you to recognise chicken by taking you round the poulterers!

This is hackneyed. The point of this note is to draw attention to some words in Plato's *Phaedo* which offer an interesting parallel. The words occur in 73 a 9-10. Kebes is expounding the argument for immortality from our alleged capacity for *anamnêsis* or recollection in the form in which (in fact) it occurs in the *Meno*. He says that "if you question men cunningly you can get them to tell you themselves how everything is" (he goes on to mention geometry, so no doubt "everything" is hyperbole). Then he uses the words: "Yet



they could not do this were there not present in them knowledge and *orthos logos*." Now it may be said that all that Socrates' performance with the slave in the *Meno* proves is that the slave had powers of sound reasoning, and that therefore all that had to be "present in him" is understanding and sound reasoning—*orthos logos* therefore equals *recta ratio*. But although this is a fair comment for us to pass on the *Meno*, it cannot be the comment that Kebes means to pass, and "understanding and sound reasoning" cannot be the translation of his "*epistémē* and *orthos logos*". If the argument is to prove pre-existence (as Kebes takes it to do), it must do so by assuming that the ability of the questionee to give the right answers depends on pre-natal acquaintance with the relevant facts. That the slave should be able to reason correctly does not imply that he existed before he was born, what shows this is the fact that it could at any time have been demonstrated by cunning questioning that he had the right answers within him, i.e. that he *had already learnt* the right answers. (Indeed the *Meno* makes some play with the perfect tense of the verb *to learn*, 86 a.). Now it may well be that Plato had come to think that this argument was crude (and this may be the reason why in the *Phaedo*, loc. cit., Socrates goes on to suggest "what may be a more convincing form" of the argument from recollection), but this does not alter the fact that he cannot intend that Kebes, in his exposition of the "less convincing" form of the argument, should mean "sound reasoning" by *orthos logos*. It must be Kebes's point that a man could not answer correctly unless he had at some time become acquainted with the right answers. We must therefore construe his phrase "knowledge and *orthos logos*" in terms of pre-natal acquaintance with the facts, and therefore *orthos logos* must mean "a correct account" of whatever it is. Here then is a case where *orthos logos* has no tendency to connote "principle" or anything of a general kind, but where it stands for something entirely particular. Any suggestion that the slave does what he does by applying general principles of any kind ruins the argument, which depends on pre-natal acquaintance with particular facts.

An obvious counter to my argument is that Plato's habitual looseness of expression is greater than to permit one to infer from his choice of a phrase in a given logical context that the phrase is able to bear the meaning which the context requires it to bear. But unless one is willing to use this counter, then it would seem that we have here a use of *orthos logos* to mean "correct account of this matter", and that this would therefore be a parallel to the use which we thought we found in the *Nicomachean Ethics*.

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## A NOTE ON OBVERSION

RECENTLY a logic has been proposed that admits singular terms that denote nothing.<sup>1</sup> In such a logic there is a certain problem regarding obversion

Suppose that statements of the form 'fx' are interpreted in such a fashion that they are true if and only if the term that plays the role of 'x' denotes something to which the term that plays the role of 'f' is applicable, but false if this condition is not fulfilled.

If now we have a singular term that does not denote, such as 'c' for Santa Claus, then the following two sentences are not materially equivalent.

$$\sim Mc \quad (1)$$

$$\hat{x}(\sim Mx)c \quad (2)$$

By the above agreement concerning a necessary and sufficient condition that a statement of the form 'fx' be true, (2) is false, since by assumption 'o' denotes nothing, and hence denotes nothing to which ' $\hat{x}(\sim Mx)$ ' is applicable. By this same agreement, and the further one that a statement is true if and only if its denial is false, (1) is true.

It appears that a logic that admits singular terms that do not denote, must either repudiate at least one of the two agreements above, or not allow obversions from a premise such as (1) to a conclusion such as (2).

A logic that allows such non-denoting terms can without harm permit an inference from (2) to (1) since in this case the premise is false if the singular term in question does not denote, while the inference is valid in the usual way if that term does denote.

The difficulty of disallowing an inference such as that from (1) to (2) can be met by adopting Quine's device of construing all singular terms as abbreviations for definite descriptions. If this is done, the scopes of the definite description abbreviation 'c' are such that (1) is true and (2) is false. This device will not, however, meet the following difficulty which is very similar to the one above concerning obversion.

There are many dyadic relations (such as 'believes in', 'worships', etc.) with which it appears that one can form many true statements having at least one argument that is a singular term that denotes nothing. Let us assume that 'Pericles worshipped Zeus' is such a statement. This sentence might with obvious abbreviations, be written

$$Wpz. \quad (3)$$

<sup>1</sup> *The Logic of Existence* by Henry S. Leonard, Philosophical Studies, vii, no. 4 (1956), 49-64.

By a logical principle very similar to that by which one passes from (1) to (2), we may pass from (3) to :

$$\hat{x}(\overline{Wpx})z. \quad (4)$$

If we still hold to the first agreement above concerning the conditions under which a statement of the form ' $\hat{x}$ ' is true, then (4) is false since ' $z$ ' denotes nothing. If the device of taking all singular terms as abbreviations for definite descriptions is adopted, then (3) and (4) have the same truth value, and all such statements as (3) are false.

R. M. JONES

## SALVAGING THE "NOOSPHERE"

THE critical notice by Professor Medawar of Teilhard de Chardin's *The Phenomenon of Man* (MIND, lxx, January 1961, 99-106) was a blockbuster which reduced its target to a pile of rubbish. I have no desire to advocate a rebuilding operation. But I would like to propose that one item might be salvaged from the ruins and made available for use in connection with evolutionary theory. I refer to Teilhard's concept of the "noosphere."

The background of my proposal is the following. It can be accepted as a historical fact that one species of animal on the earth, *Homo sapiens*, has outstripped all others in the magnitude of the changes which have occurred in its way of life during the last 50,000 years. The changes are usually said to constitute the process of cultural evolution. It is also a fact that there exists at present no comprehensive explanatory theory of that process. The best we have are a few clues about some of the causal factors (e.g. tool-using, tool-making, communication by speech and language, etc.) which determined the course of human evolution from the Paleolithic period onward.

Under these circumstances it is desirable to have available a number of theoretical models in the light of which explanatory hypotheses can be formulated. Thus is a familiar state of affairs in the natural sciences when they are in a formative stage, as the sciences of man certainly are. Now it seems to me that the concept of the noosphere, freed from the mystical associations of *The Phenomenon of Man* and given a certain degree of precision, might serve as a useful model for anthropologists, sociologists, and psychologists who undertake to theorize about cultural evolution.

What does the model amount to? Briefly, it is built on the classical representation in geology of the earth as a sequence of concentric, spherical shells or envelopes—barysphere, lithosphere, hydrosphere, atmosphere and biosphere. The last of these, introduced by the geologist Suess, was designed to represent the envelope of organic matter which originated and spread around the globe during the Pre-Cambrian era. What Teilhard proposes is that we regard the process of cultural evolution as having generated another planetary envelope, distinct from but superimposed on the biosphere, a "sheet of humanized and socialized matter" which he calls the "noosphere." The title seems reasonably apt since the noosphere is exclusively the product of *Homo sapiens*, and embraces not only technological but also intellectual and social creations. Viewed historically, the noosphere is the *ensemble* composed of evolving man and his various cultures.

There seems to me no intrinsic reason why this model should be associated with the dithyrambic and irresponsible language which Professor Medawar rightly condemns in his review. On the contrary, the model might enable the sciences of man to speak in fruitful

ways about the process of cultural evolution, and so to move towards filling in some theoretical gaps. If we are ever to have a unified theory of evolution we need all the ancillary devices which look promising. The noosphere strikes me as one such device and hence worth salvaging from the debris of *The Phenomenon of Man*.

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# COROLLARY TO GOODMAN'S EXPLICATION OF 'ABOUT'

For Professor Goodman,<sup>1</sup> "*S* is absolutely about *k* if and only if some statement *T* follows from *S* differentially with respect to *k*", where 'a statement *T* follows from *S* differentially with respect to *k*' if *T* contains an expression designating *k* and follows logically from *S*, while no generalization of *T* with respect to any part of that expression also follows logically from *S*'. We answer here a question which Goodman leaves open, showing that if a statement *S* is absolutely about *k* its negation must be also.

Now regardless of its complexity, *S* can be expressed set-theoretically. But since differential implication turns only on the logic of quantification and identity, we may signify membership by a 2-place predicate '*E*' governed by no special axioms (Example 'The class of cows is amorphous and Hermione and Sappho are distinct cows' may be represented as ' $\exists x \forall y (x E y \supset x = y)$ '.) Thus representation of *S* as a quantificational schema (possibly with identity) suffices for consideration of all questions of differential implication. We shall follow Quine<sup>2</sup> in understanding *implication* as *validity* of the material conditional, so that *T* follows logically from (is implied by) *S* just in case  $S \supset T$  is valid.<sup>3</sup>

It will therefore suffice to show that if a quantificational schema *S* implies some schema *T* without implying  $(y)T$ , then there is a schema *R* such that  $\neg S$  implies *R* without implying  $(y)R$ . This is equivalent to the following. If (1)  $\neg S$  implies the universal quantification with respect to *y* of every schema which it implies then (2) *S* does also. Now if (1) then  $\neg S$  must in particular imply  $(y)\neg S$ . But in this case *S* implies  $(y)S$  (by an argument given below), and then if *S* implies *T* we have the validity of  $S \supset (y)S$ ,  $S \supset T$ ,  $(y)(S \supset T)$ ,  $(y)S \supset (y)T$ , and  $S \supset (y)T$ , i.e., the implication of  $(y)T$  by *S*.

To complete the proof we show that  $S \supset (y)S$  is valid if  $\neg S \supset (y)\neg S$  is. If  $\neg S \supset (y)\neg S$  is valid so must be  $(y)(\neg S \supset (y)\neg S)$  and its equivalent  $(\exists y)\neg S \supset (y)\neg S$ . If  $S \supset (y)S$  were nonvalid there would be an interpretation *J* making *S* true and  $(y)S$  false. Then *J* would make true  $(\exists y)\neg S$ ,  $(y)\neg S$ , and  $\neg S$ , a contradiction.

The argument can be extended to cases in which logical truth (and hence differential implication) is construed in terms broader than those of quantificational validity.

<sup>1</sup> Nelson Goodman "About", *MIND*, lxx (January 1961), pp 1-24. I am indebted to Professor Goodman for showing me his manuscript prior to its publication.

<sup>2</sup> W. V. Quine, *Methods of Logic* (New York, Holt, 1950).

<sup>3</sup> We use the obvious use-mention conventions according to which ' $S \supset T$ ' names the conditional with *S* as antecedent and *T* as consequent.

## VIII.—CRITICAL NOTICE

*The Phenomenological Movement: A historical introduction.* By HERBERT SPIEGELBERG. The Hague: Martinus Nijhoff. 1960. (Collection *Phaenomenologica*, n. 6). 2 vols. Pp. xxxii + 735.

THIS is a book which is long overdue. Mr. Spiegelberg has attempted to write a history of the Phenomenological movement which will also serve to provide some elements of a definition. He provides a series of related studies of the major, and many of the minor figures of the Phenomenological movement, crowned with an attempt, in the last chapter, to outline the phenomenological method as it emerges out of this history.

The coverage is remarkably complete and at the same time very fair-minded and accurate. All the major figures are treated at some length—Husserl himself, Scheler, Heidegger, as well as the fore-runners Brentano and Stumpf and the major French practitioners, Sartre and Merleau-Ponty. While each figure is studied principally as a phenomenologist—whether or not phenomenology is or has remained central to his work—Spiegelberg has steered admirably clear of the obvious attendant danger—the “touching up” of the thought of the various protagonists so as to make them sit more happily in the same tradition. On the contrary, each figure is understood scrupulously in his own terms, and given a chance, as it were, to bid for our respect. Spiegelberg’s fair-mindedness even holds good in face of the sordid political behaviour of Heidegger in the early Nazi period and his shabby treatment of Husserl. This is mentioned in passing with the greatest restraint and matter-of-factness, a biographical fact which is useful in understanding Heidegger—Spiegelberg throughout provides the indispensable skeletal facts about the major figures—but not the occasion for the comment that most of us would have found irresistible.

But on putting down this two-volume work one is almost left with the impression that Spiegelberg’s very accuracy has defeated his second purpose—providing us with an idea of what phenomenology is. In fact the thinkers studied are so disparate that it is difficult to see the thread of unity underlying all their work. The picture is further complicated by the fact that for some phenomenology is only part of their work, or only occupied them for a period of their development (e.g. Scheler, Heidegger). Spiegelberg, therefore, opens his last section, designed to give the reader some guide as to what phenomenology is, with some embarrassment:

The preceding account of the Phenomenological Movement could easily have given the impression that all there is to phenomenology is its history as expressed in the multifarious and fluid ideas of sundry phenomenologists. Such an impression even contains a considerable amount of truth (p. 653).

The reader might be tempted to conclude that this is the whole truth, but Spiegelberg believes that some common elements can be

abstracted. After rejecting the course of defining phenomenology by a summary of its results he singles out as the 'characteristic core' the phenomenological method. The rest of the chapter is devoted to a clear and detailed outline of this method.

But a question remains. is it possible to define phenomenology simply by the phenomenological method, that is, in abstraction from certain beliefs about the adequacy of this method to solve certain perennial philosophical problems? Can one abstract the method from the doctrine which alone can induce one to practise it? Mr. Spiegelberg's discussion does not seem to yield a definite answer on this point. On one hand he starts to outline the doctrinal basis of the method in a section entitled "The phenomenological method as a protest against reductionism" (pp. 656-658). But this outline is left incomplete. Some doctrines incompatible with phenomenology are identified but not its aims and aspirations. Thus it is made clear that phenomenology opposes the principle of Occam's razor on the level of the phenomena, it may be laudable to cut down the number of explanatory hypotheses to the minimum possible, but this principle cannot hold good for the phenomena of experience—if one's aim, that is, is to discover the nature of that experience. Similarly a protest is registered against the traditional empiricism in its account of the nature of experience precisely in that it has not been empirical enough in that it has proceeded too much by reasoning as to what we *must* perceive according to a certain theory about the perceiving process, and too little by an actual examination of what perception is really like. Thus Mr. Spiegelberg singles out for attack, *e.g.* the "sense-organ bias", the principle that "nothing is to be recognized as a datum unless it can be assigned a specific sense-organ (in the biological organism) as its receptor" (*loc. cit.*).

Now one might easily accept these criticisms as valid and take the corresponding warnings to heart without adopting the phenomenological method. Thus to continue the above example, one might refuse all traditional a priori doctrines about experience and yet not turn to the intuiting of the phenomena as the means of access to a correct view. One might, for instance, turn to indirect experimental methods or to reasoning from the structure of empirical language. These methods may be mistaken but they are not excluded by the simple fact that one rejects traditional empiricism. The rationale for phenomenology must include more than the rejection of reductionism.

But Mr. Spiegelberg does not expand further on the rationale for the method. And perhaps this is because to expand further would be to go beyond the area of agreement between the different practitioners. But if there is no agreement among phenomenologists as to the rationale behind their procedure, then we cannot speak of *the* Phenomenological school. It is doubtful whether we can even speak of a procedure or method common to the different members.

One is strongly tempted to draw this last conclusion from Mr. Spiegelberg's account of "the method". He outlines seven steps, of



which the first three are said to have been "accepted, at least implicitly, and practised by all those who have aligned themselves with the Phenomenological Movement" (p 659) The later ones are only accepted by a smaller group The steps are .

1. investigating particular phenomena
2. investigating general essences
3. apprehending essential relationships among essences
4. watching modes of appearing
5. watching the constitution of phenomena in consciousness
6. suspending belief in the existence of phenomena
7. interpreting the meaning of phenomena

All these steps are discussed in some detail, with an illustrative example running throughout, the study of the phenomena of force

Now steps two and three—or at least the latter—do not seem to be peculiar to phenomenologists They seem to be coterminous with the procedures of philosophical study and argument as these have been practised since Socrates The characterization of what is being done (in this case "investigation of essences") is particular to this school, but this by itself need not alter the way in which this study is carried on Mr Spiegelberg's illustrative findings concerning force seem to confirm this The investigation, he says,

might lead to such insights as essentially force is extended over an area, essentially it has intensity; essentially it can increase or decrease; essentially, it can never reach an absolute maximum (p 681)

But such conclusions might also be reached by someone who claimed to be examining the concept 'force', or the "form" of Force

Steps 1, 4 and 5 are clearly in line with phenomenology as a careful intuitive study of the phenomena of experience, and step 6 is central to Husserl's philosophy. But precisely here it becomes dubious whether these are generally practised or whether, even where there is verbal agreement over practice, the different thinkers are really talking about the same thing Thus Mr Spiegelberg's account of step 6 (the *epoché*) seems to relegate it to the status of an accessory but not indispensable help to the earlier ones, that is, it may help to put us in the right frame of mind, prising us free from our presuppositions, but this is hardly what the *epoché* meant for Husserl who made it the gate of entry to the whole transcendental field and thus to the very centre of phenomenology With this difference in aim, can it be said that we are still talking of the same procedure?

Step 6 is, of course, not one of those which Mr Spiegelberg would claim are common to all phenomenologists; but even when we turn to step 1, which alone among the first three seems peculiar to phenomenology, one can raise doubts about its generality It is highly doubtful whether one can say that, e.g. Merleau-Ponty or the post-war Sartre have practised this as it is described here

And as for step 7, it is frankly confined to those thinkers loosely called "existentialists".

At this point one might seem forced to the negative conclusion which Mr Spiegelberg wishes to avoid, that there is little in common among phenomenologists save the name. But this would probably constitute an equal and opposite over-statement. In fact the return to intuitive study of experience has played a role in a number of different philosophies and has served to advance a number of different philosophical concerns, but these are not without some connection with each other, and the movement can be seen as developing in a certain way. What one can take issue with is the thesis that the phenomenological movement has shown continuity in its research, applying the same method to wider and wider fields of phenomena and attempting to accumulate results of an agreed kind, that, in short, it has behaved as the type of movement of research that Husserl envisaged. I am not sure that this is Mr Spiegelberg's thesis, but some of the things he says seem to give the impression that the movement can at least be usefully looked at in this way (e.g. pp 644 ff). At any rate, this strong thesis seems to me to be unfounded.

In fact the return "to the things themselves", to the intuitive study of ordinary experience, has played many roles. First it has played that of a liberating idea which re-opened the road to philosophical research in what to English philosophers should be a perfectly straightforward sense, although carried on under the foreign banner of "intuition of essences". The history of philosophy seems to show that philosophy itself is constantly in danger of being illegitimately absorbed into some related positive study or of becoming the prisoner of some blinding presuppositions, and therefore to stand in need of some liberating idea which by providing a radically new (relatively speaking) account of the subject enables it to begin again. Just as the idea of the Forms enabled Plato's Socrates to break away from the stultifying debates between Eleatics and Heraclitans, and the intuitions about language as a tool and language-games enabled Wittgenstein to break with logical atomism, so phenomenology was the road by which Husserl broke with psychologism, the imperialism of genetic psychology, which seemed to threaten logic and philosophy in the Germany of the time.

To some this seems to have been the main place of phenomenology in their thought. Thus of Husserl's early disciples and sympathizers in the first decade of the century, many did not follow him into the further reaches of transcendental phenomenology. But for Husserl himself liberation was just a stage. He went on to conceive of phenomenology as an essentially new beginning in epistemology, and as a way to make philosophy a "rigorous science" to begin from zero without presuppositions and build up a body of firm knowledge. This lay behind Husserl's growing invocation of Descartes as a forerunner.

In this period, which begins roughly with the *Ideen*, published in 1913, Husserl attempts to go beyond a mere study of "essences" of the concepts of empirical description and logic, and to discover by

intuitive study how they arise and are formed, how they are "constituted" in consciousness. To describe the enterprise in terms of its relations to Kantian philosophy—a relation of which Husserl was more and more aware—one could say that he attempted to submit the "concepts of the understanding" themselves to intuitive scrutiny in order to study their genesis. The point of the *epoché* was to open this new field of study. The idea behind the "bracketing" or the suspension of belief in existence was not that of Descartes, *i.e.* to discover what could resist the solvent of doubt and thus be said with certainty to exist. This would be to remain still in the "natural attitude" in which consciousness was considered another "substance" alongside external objects. This point was a "conversion" from all questions concerning existence to a pure study of the phenomena and their constitution.

This long journey into transcendental subjectivity seems to have been designed to permit a return, enriched with a firm foundation for knowledge, with a resolution of the "crisis" of science. But the enterprise does not seem to have succeeded. Husserl, so to speak, never came back.

Thus phenomenology has been used as a liberating idea and as the foundation for a new epistemology. With modern French phenomenology we seem to have yet a third type of philosophical concern, the attempt to work out an adequate philosophical anthropology. For Sartre the central problems seem to centre around the relation of subject to object, the problem of human freedom, with Merleau-Ponty the aim has been the discovery of a third way between the mechanistic notion of man which flows from "scientism" and the idealistic dualism which has often been opposed to it. This is why French phenomenologists have shown an intense interest in Hegel and the young Marx—even to the point of claiming to see more than a verbal resemblance between the *Phenomenology of Mind* and Husserl's work—while Husserl had little time for these authors. For Merleau-Ponty the point of phenomenology or the return to everyday experience is not to open a new field of research but to build from the fact and nature of unreflective experience the notion of man as "existence" or "*être-au-monde*", which is neither materialist nor idealist. Thus while Merleau-Ponty agrees with Husserl that phenomenology is a return "to the things themselves", a return from the type of scientific theory which tells us what human experience must be like in virtue of some explanatory theory, he does not believe that the correct view is established as correct on the evidence of intuition, but rather by philosophical argument, *e.g.* from the definition of consciousness as "intentional" or from scientific findings. Thus Merleau-Ponty's first work, *La Structure du Comportement*, was an attempt to define his theory of living organisms, arguing from the results of psychology and biology, and there is nothing specifically phenomenological about this procedure. In Merleau-Ponty's view it would seem, the phenomenological

conversion, a fresh look at the facts of experience free from scientistic prejudices, is essential if one is to understand his philosophical anthropology, but the probative weight of this theory does not rest on the intuition thus gained

Of course the philosophy of Merleau-Ponty cannot be identified with that of French phenomenology as a whole. But it is a significant fact for the history of the movement that the dominant figure of this school, and indeed of all French philosophical thought in the post-war period, should have travelled this distance from Husserl. While for the latter phenomenology is an epistemologically privileged starting point opening the way to a rigorous science, for the former it is a conversion which is justified afterwards in the comprehension of the human condition which it makes possible. At this point it becomes difficult to see these two figures as labourers in the same vineyard pushing forward on the same line of research. It is not only that the central concerns have changed, the very idea of what phenomenology is has changed as well. It would be misleading to say that they held in common the phenomenological method, because for Merleau-Ponty and many of the French school, phenomenology can no longer be characterized as a method; it is more in the nature of an orientation or starting point.

There seem to be grounds therefore for questioning Mr Spiegelberg's final interpretation. It may be more accurate to see the phenomenological movement as having undergone a far-reaching shift in direction—roughly with Heidegger or Sartre—which has broken the line of continuity from Husserl. If this is true then one would also see the future of the movement in a different light—if indeed, it has a future—as lying less in a continuation of Husserlian research than in a working out of the basic orientation of the French school. But whatever the future of the movement, Mr. Spiegelberg's comprehensive and careful account is essential to an understanding of its past.

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CHARLES TAYLOR

## IX.—NEW BOOKS

*Irrational Man—A Study in Existential Philosophy* By WILLIAM BARRETT. Heinemann, 1960. Pp. 278 21s

SOME British and American philosophers still regard existentialism as a philosophical disease and it seems to be the case that the academic doctors in this part of the world are still at the stage of receiving second-hand descriptions of the symptoms and, hence, making doubtful diagnoses. There have been notably few attempts to open up the patients and thoroughly explore the anatomical complications. One current diagnosis is that the writings of philosophers such as Heidegger and Sartre result from philosophic aphasia, producing a complete breakdown in the communication of their philosophical opinions, another is that they suffer from compulsive abuse of language. Occasionally one hears the more sophisticated view that existentialism is locked up in Cartesian subjectivity and mistakes rhapsodies on the vagaries of personal experience for philosophic utterance. Whatever the justice of these criticisms, they all fail to attack the writers concerned in those parts of their work which have something in common with our home-grown philosophical writings, namely, philosophical problems and perplexities.

Consider, for example, the problem of the existence of other minds as treated by Mr Strawson in chapter 3 of his recent book *Individuals*. Sartre, in *L'Être et le Néant*, devotes some eighty pages of exposition and argument to the existence of others. I think it is clear from these sources that there is a philosophical problem in common between the two writers. Furthermore, it is worth noting that Sartre and Strawson agree in rejecting certain answers that have been given to the question, "How do I know of the existence of other minds?" For example, they deny that we know of this because we argue analogically from what we know of our own mind and behaviour and also that we arrive at this conclusion by an inductive argument that leaves us with only a probable conclusion to the effect that other minds exist. This is not the place to argue that Sartre and Strawson offer the same or similar solutions to this problem—for one thing, the idiom is very different—but when Strawson says:

"... we see others as self-ascribers, not on the basis of observation, of what we ascribe to them on this basis", he seems to be saying something close to what Sartre holds in saying:

"... I organise him (the other) in the midst of the world in so far as he organises the world towards himself."

Again with relation to Sartre we could make out a similar point for affinities between Professor Hampshire's views on freedom as expressed in *Thought and Action* and the view of free action that Sartre develops in the later parts of *L'Être et le Néant*.

Consider, finally, Miss Anscombe's account of the knowledge we have of what at any given moment we may be doing in the sense of "doing" which presupposes that we are doing it on purpose or intentionally. This knowledge, she suggests, we have "without observation". Briefly and crudely, we do not have to look and see what we are doing in this sense of "doing". Sartre has as one of the central pillars of his argument in *L'Être et le Néant* an analysis of this kind of knowledge which he concludes is "pre-reflective" and quite different epistemologically from that "reflective" knowledge which constitutes the natural sciences. Once again, there is common ground here worth investigation.

In brief, it seems to me that what we now require are not blanket generalisations about existentialism or apologetic introductions to it, but rather patient and painstaking examinations of particular thinkers in the tradition, carried on with an eye to the dominant concerns of British and American philosophy in this century. Too much exegesis of existentialist thought has set out to edify us with the insights of thinkers such as Heidegger and Sartre, to inject some good red blood into the allegedly anaemic body of English philosophy. What should be realised is that analytic philosophy is not so anaemic as is often supposed, nor does existentialism consist entirely of sound and fury signifying little that is of philosophical value.

These matters have been mentioned to explain a sense of disappointment experienced after reading Professor Barrett's book. This is not because it is a bad book of its kind but rather because of the kind of book that it is. At its best it might be said to be an introduction to the subject and at its worst good philosophical journalism. It certainly belies the publisher's claim, printed on the dust jacket, that the book contains "a major and original philosophic statement". There is next to no textual analysis, references for passages cited are rarely given and, unlike most introductions to most subjects, there is no bibliography nor any suggestions for further reading. The style is diffuse and a love of metaphor often obscures thoughts that would be more clearly expressed literally.

The book consists of a series of essays on leading themes found in that group of writers usually associated with existentialism, namely, Kierkegaard, Nietzsche, Heidegger and Sartre, together with sidelong glances at the history of philosophy and contemporary cultural manifestations often associated with this group of writers. Jaspers is accorded no independent treatment.

Barrett begins with an essay in the sociology of philosophy which contends that existentialism, in contrast with the prevailing "positivistic and analytic" schools of "academic philosophy", is "able to cross the frontiers from the academy into the world at large". Existentialism, according to Barrett, is in revolt against the oversimplified rationalism of positivism and Marxism, seeking to bring before us "all that is dark and questionable in man's existence". He then proceeds to cite three main factors in the growth of this philosophical *genre*; they are the decline of religion, the rational ordering of society (in Weber's sense of 'rational') and the discovery, symbolised in Gödel's theorem and the principle of complementarity in atomic physics, of "a finitude at the heart of science". Barrett then proceeds to cite "the testimony of art", seeing in the work of Hemingway, Joyce, the Dadaists and primitivists an impatience with order, abstraction and proportion and a predilection for chaos, concreteness and asymmetry. The chief defect of this part of the book is its ready indulgence in facile generalisation where matters of complex cultural analysis are concerned and in the adoption of too simple a theory as to the symbolic significances of art. It would be just as possible to argue *contra* Barrett that, for example, Nietzsche and Kafka seek in their prose a perfection of form and expression unrealisable in other spheres and to this extent are not impatient with order and proportion. One might also cite in this connection the careful construction of certain of Sartre's novels and plays. It has been equally characteristic of Western art in this century to be obsessed with problems of form, and this has not been because say, in music, tonality has "broken down", but rather because artists have sought the delineation of new forms. "The dialectic of the expected and

the surprising in art, leading into the construction of new forms and frameworks, in their turn to be expanded and experimented with, is a constant feature of the history of art and is not peculiar to this century. Nor is it correct to say that, 'Modern art has discarded the traditional assumptions of rational form'. 'Worthwhile art has never lived by any diet as thin as this and I cannot see that the form of Faulkner's *The Fable* or Kafka's *The Castle* is any more or less 'rational' than that of, say, *Tristram Shandy* or *The Small House at Allington*. Faulkner certainly does differ from Sterne and Kafka from Trollope but it is not helpful to describe the difference as one of more or less rationality of form.

One final lesson to be drawn from this section of the book concerns the entirely respectable discipline I have already referred to as sociology of philosophy. Anthropologists go and live with communities before they proceed to expound the nature of the cultural patterns to be found there. Living with a philosophical community seems to me to be an indispensable pre-requisite of analysing it sociologically. One does not gather from this first section of Barrett's book that his acquaintance, in this sense, with positivism and analytic philosophy is anything more than the recollection of travellers' tales.

The second part of the book where Barrett deals with the sources of existentialism is more convincing. Beginning from Matthew Arnold's distinction between Hebraistic and Hellenistic forces, existentialism is regarded as providing the necessary antidote to the cool and complacent rationalism of the Hellenistic tradition. Barrett claims that traditional philosophy, particularly where it has been theological, has been under the dominance of the Hellenistic tradition. Here Barrett draws very obviously on what one can take to be Heidegger's recent interpretations of Plato as having substituted for the notion of truth as revelation of being the notion of truth as concept or proposition. The chief defect of this way of looking at the history of philosophy is that it classes together philosophers who ought to be kept apart. For example, where is Hume to be put on this plan? He seems to be both a rationalist in that he reasons and an existentialist in that he is sceptical about reason. There is an existential flavour to Hume, but it lies not in him being on one side or the other of Barrett's dichotomy, but rather in Hume's recognition of a tension between the claims of reason and tendencies of another sort.

Barrett is more successful with the Christian sources of existentialism. He finds these in the paradox of Tertullian, St Augustine in the *Confessions*, those theologians who have asserted the primacy of will over intellect and inevitably, the writings of Pascal. In this section Barrett interestingly links the Sartreian tag, "Existence comes before essence", with the Thomistic view that the being of anything consists in its *actus essendi*. Still, two points require to be made about this. The first is that the tag comes from a popular lecture now repudiated by Sartre as an accurate account of his position and must be viewed in this light. Secondly, in so far as it affords a clue to the interpretation of Sartre's thought it is intended to apply not to being in general but to the being of the *pour-soi* as distinct from the being of the *en-soi*. This distinguishes Sartre's view from the Thomist one very sharply, for in the latter case the point belongs, I presume, to an account of being in general whereas in the former case it is the basis of Sartre's fundamental dualism.

Barrett finds a perpetual tension in the history of Christian theology between an emphasis on "the vital" and "the rational" in man. The existentialists, in Pascal's terminology, side not with the God of the

philosophers but with the God of Abraham, Isaac and Jacob. It is surprising in this connection that Barrett does not make more of the influence of existentialist thought upon contemporary theology. Kierkegaard, Buber, Marcel, perhaps Tillich and others are surely in part responsible for the rooted prejudice against any kind of natural theology and rational elaboration of Christian faith.

The best parts of the book are the chapters in which Barrett presents thumbnail sketches and interpretations of Kierkegaard, Nietzsche, Heidegger and Sartre. But here too there are mistakes and reckless generalisations. For example, Kierkegaard is represented as having made "the first radical reappraisal of the subject of truth" since Aquinas's *De Veritate* This issued, according to Barrett, in Kierkegaard's discovery that religious faith was a way of being rather than a matter of statements in which one professed to believe. This seems curious, for what is presented as a new account of the concept of truth turns out to be an account of what it is to be religious as distinct from what it is to have religious belief. Furthermore, it is at least arguable that a necessary condition of being religious is having some minimal beliefs properly describable as religious beliefs.

The chapter on Nietzsche interprets him mainly from the standpoint of *Ecce Homo* and *Also Sprach*. Barrett correctly stresses the complexities of Nietzsche's atheism and, despite first appearances, the systematic nature of his thought. The meaning of "the will to power" is represented as Nietzsche's ironical comment on the dynamism of technological society, an interpretation due, it would seem to Heidegger. Once again little by way of textual evidence is cited to support the interpretation.

With Heidegger and Sartre, each of whom gets a chapter, Barrett comes to two thinkers in whom the autobiographical urge is much less obvious than in the cases of Kierkegaard and Nietzsche. Heidegger is presented as "digging himself out" from the ruins of Nietzsche's philosophy of power. The chief objection to this philosophy, according to Heidegger, is that it is a product of the obsession of men with particular things which they strive to know only that they may master them and transform them into instrumentalities. Against this concern with particular things as instruments Heidegger recommends a return to being itself. I do not profess to understand this recommendation, though it appears to have affinities with Parmenidean monism. Heidegger has expounded his views chiefly in two ways; the earlier method approached being through an analysis of human being carried out in a broadly phenomenological way. It is this approach, which mainly through *Sein und Zeit*, has had the greatest influence on his contemporaries though according to Heidegger himself this work has been taken wrongly as a philosophical anthropology by many of its readers, whereas it was intended as a prolegomena to ontology. Heidegger's later attacks on the problem of being itself take the form of interpretations of philosophical utterances—mainly pre-Socratic—in order to reconstitute the attitude to being of those whose understanding of being had not been corrupted, as ours has, by the Platonic conceptual smokescreen. Barrett makes much of the idea of a pre-conceptual understanding of being, but he never considers the obvious objection to this notion, that it constitutes a contradiction in terms. Rather than to talk of a preconceptual understanding of being, it might be more accurate to speak of acquaintance with being.

An important part of Barrett's discussion of Heidegger concerns his views on language. Barrett opposes Heidegger's views on language to "the various forms of semanticism now in vogue in this country and in





The second of the appendices questions, "... one of the more entrenched dogmas of Positivism and Analytic philosophy", namely, that existence is not a genuine predicate. Barrett correctly points out that on the Russellian analysis what disappears as a predicate in "Socrates exists", reappears in "Ex( $x = \text{Socrates}$ )" as a verb of sorts. He then argues that the attempt to dispense with the copula in formal languages requires that one be able to specify times numerically and by co-ordinates in order that one can get rid of tensed verbs involving the copula. He rules out this possibility on the grounds that doing this presupposes something like a Newtonian absolute time and hence concludes that the copula is an essential part of language. Barrett builds on this a further contention that the possibility of tensed verbs rests in turn upon our ability to say of anything that it is or is not *now*. The existence of the tenseless "is", beloved of the logicians as in "7 is a prime number", Barrett takes as showing that the tenseless "is" occurs only in statements that are not about existence as, for example, mathematical statements. He concludes, somewhat rhetorically, "so far as he logicizes, man tends to forget existence. It happens, however, that he must first exist in order to logicize".

This aphorism sums up Barrett's account of existentialism. He approves of it as an attempt to grasp the nature of the priority to which it refers. In so far as Anglo-Saxon philosophy has been dominated by paradigms derived from the natural sciences, Professor Barrett is probably correct in holding that existentialism can provide a needful corrective to much philosophy written in English. On the other hand, he neglects the correctives that have been applied from within this tradition. In any case I doubt the usefulness of referring largely to such enormous strawmen as existentialism and analytic philosophy. If philosophers in this country and in the United States are going to learn more from existentialism than they have up until now, then what is needed are more detailed studies of existentialist contributions to common philosophical problems and not more general accounts, however stimulating they may be.

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*The Problem of Value* By A C GRAHAM Hutchinson University Library.  
London, 1961 Pp 192 12s 6d

THE aim of this lively and interesting little book is to attend, in a manner the layman can appreciate, to an important question by which he is often troubled but to which contemporary British philosophy does not give a helpful answer, namely, 'By what standards shall I live?' The task is soon found, however, to involve extensive use of linguistic analysis, and in the concluding chapter the writer declares that he has not really answered this important question, but has merely elucidated the rules and canons by which a sound answer to it would have to abide. That is excessive modesty. This book would be much less stimulating and provocative had not its author frequently gone well beyond linguistic analysis. The hungry sheep looking up to him do get fed (and strong meat a lot of it will be found by some, e.g. pp 82-84, 165-171, etc.) even though many of his handouts really are just recipes.

The main theme is the enunciation, in Part I, of the principles of critical judgment appropriate to "prescriptive uses" (p 18) of language, and their application, in Part II to Morals, in Part III to Aesthetics, and in Part IV

to Metaphysics Religion, Myth, and Mysticism. In chapter 2 of Part I. Graham's main aim is to refute the claim that from descriptive premisses alone (pp. 24-25, 28-30) we cannot deduce objective standards for prescriptions including moral judgments. His main target here is Hare whose view that reasons for prescriptions are still further prescriptions he rejects on the grounds that it would involve an infinite regress arrestable. Graham thinks, only by a descriptive statement of a certain sort, namely, a statement about what the person in question likes or wants

In chapter 3. Graham formulates the principles employed in judging the merits and demerits of the reasons someone may offer in support of a given line of action. Under this head, he discusses three different things, hypothetical standards, necessary standards, and necessary premisses about ends. Hypothetical standards or principles "are grounded in statements about the relations of means to end and about the wants and likings of the persons concerned" (p. 32). Necessary standards ("which have nothing in common with Kant's categorical imperative") (p. 36) are "correct *a priori*" because they are means to an individual's ends whatever his ends may be. Terms such as 'happiness' and 'freedom from worry' are names of "necessary ends, which by definition are wanted and enjoyed" (p. 34). Their analysis provides us "not so much with necessary principles as with necessary premisses about ends" (p. 34). That is to say, a person (logically) cannot but want to be happy or dislike being worried which, by Graham's simple Behaviourist definitions of 'want', 'like' and 'enjoy' (p. 31) entails his having a disposition to positive, prolonging action when he is happy, to negative corrective or escaping action when he is worried or afraid (pp. 31, 34, 35).

Of course, "sometimes we may say that a man 'really enjoys making himself miserable' . . . but the lust for misery cannot be whole-hearted, since if there were no conflicting impulse to escape this state it would not be misery" (p. 35). "Such a man struggles to relieve himself from each worry even though he looks for another as soon as it is gone" (p. 35). On the basis of this Graham goes on to distinguish two very general mutually opposed types of response to our environment and changes in it, 'Being on the side of Life' and 'Being on the side of Death'. We respond in the former way if "we are disposed to create rather than destroy, love rather than hate, be confident rather than doubt, trust rather than suspect, hope rather than fear" (p. 35), and so forth. And (other things being equal) responding in the positive way is *justified*, in the negative *unjustified*, "because the former is the more enjoyable, and the capacity to choose it is the capacity for happiness" (pp. 35-36).

Of course, it is impossible to deduce from these necessary ends and necessary standards what people's ends will in fact be at particular times or what they ought to be (p. 37). How, then, are we to choose our ends? The answer is simple. Only some ends are capable of being chosen, and in their choice we use the ordinary standards, because "ends which we choose are always means to further ends which we pursue without having chosen them" (p. 37). Another kind of ends, our "inclinations and tastes" (p. 37), are not subject to our choice: we simply "discover by self-examination" that we have them (p. 37). Such tastes also change, but their change is not the result of our choices. Of course, we can record and predict such changes in ourselves, we can even grade them as changes for the better or worse (pp. 37-38), as we can in the case of other people, but we cannot choose to have them or not to have them, as such evaluations prescribe. Thus, the reason why we need not choose our end "in

Existentialist anguish, by an arbitrary leap in the dark" (p 37) is that one kind of ends are chosen by reference to the ordinary standards, and the other kind are not chosen at all - "it is simply a fact that I want something and shall continue to want it even if I strive to obey a standard which forbids me to have it" (p 38)

In sum, the problems of judging prescriptions are reduced to answering the three factual questions of what we really want, how much we want it, and what are the appropriate ways of obtaining it. The excellence of our prescriptions depends on correctly estimating what the person concerned really wants, the proportions in which he wants these things and then pointing out the steps which will lead to the satisfaction of these wants in proportion to their respective magnitudes.

Now, Graham's theory—if I have understood it—seems to me open to serious objections. In the first place, although our tastes are indeed not subject to choice, some are subject to modification and control. If I regard as a change for the better a certain conceivable change in my tastes, say, from whiskey to orange juice, and I know of steps capable of bringing it about, then I may decide to rid myself not merely of my addiction to whiskey, but even of my desire for it and of any enjoyment I may now derive from drinking it. Thus, the fact that I cannot at will choose to have or not to have certain tastes does not necessarily place these tastes beyond control in accordance with evaluative principles. Thus, even though, as is no doubt the case, there will at a given time be certain inclinations and tastes which we cannot change, even in this indirect way, it is not by reference only to these that we decide in what direction to modify our controllable inclinations and tastes. Surely we have better reason for seeking ways of ridding ourselves of the enjoyment of smoking than of eating, even though neither change be possible at the moment. Graham's distinction between ends which are and ends which are not subject to choice therefore does not get rid of the Existentialist's embarrassment of excessive freedom.

It is a corollary of this that in choosing ends subject to our choice we can make mistakes of a sort other than the three permitted by Graham's theory. Suppose "I choose to devote my life to the ambition of becoming Prime Minister" (chosen end) "because I already want power, fame" and so forth (unchosen ends) (p 37). Then I have made the first sort of mistake if becoming Prime Minister does not give me power, fame and the rest, as I thought it would. On Graham's theory I must have made the second or third sort of mistake, "if in fact the experience of power and fame proves unsatisfying", for that shows that I misdiagnosed my real wants, that I did not really want power and fame, or not as much as I thought I did (p 37). But does this follow? Clearly, I have made some kind of mistake, but not necessarily either of the two. I need not have been wrong in *thinking* that I wanted fame and power. I may have been wrong in *wanting* them. Perhaps I was not mistaken about *what* I really wanted, but *in* really wanting what I did. Perhaps I *should* not have wanted them. The unsatisfyingness of the experience of power and fame may in fact bring it about that I cease to want them, for I may have wanted them in the first place only because I expected them to be satisfying, and they now prove not to be. In his definitions of 'want', 'like' and 'enjoy' (p 31, also p 44) and in his example of someone's choosing to become Prime Minister because he already wants power and fame (p 37) Graham quite implausibly identifies the *magnitude* of a want with the *degree* of *satisfyingness* of the satisfaction of this want. In his explanation of



and Hare. The artist's task according to Graham, is to communicate experiences. A work of art is merely a piece of canvas streaked with paint a lump of chiselled stone, a series of marks on paper: it is a tool used by the artist to make us sense, imagine, think and feel in certain ways (p 95 also p 156). The critic's task is aesthetic valuation, that is the valuation of every kind of communicable experience (p. 123). Where such communication is attempted by means of words, they are not used descriptively, but to make us see things and otherwise sense them in imagination (p 121). A critic cannot perform his task without responding to the work of art he criticizes (pp 100, 101). The critic therefore must have three qualifications. He must be sufficiently sensitive for works of art to make their impact on him. He must be sufficiently knowledgeable to discover, by an analysis of the work of art, how the artist achieves his effect and he must know and be able to apply the critical standards appropriate for a work of art, as opposed to propaganda and entertainment in assessing its objective merit. The hoary philosophical question 'What is Art' is not therefore, a request to state that peculiar quality possession of which makes an artefact a work of art, but a request to elucidate the standard used by a critic and to show which of these are 'mere reflections of his personal prejudices and which have objective justification' (pp 83 137-138). Accordingly, Graham distinguishes three different kinds of maxims: (i) technical maxims, such as 'writers should prefer the exact word to the euphonious one'. These are guides for the artist not standards for the critic. From the spectator's point of view, they explain why an artist succeeded or failed in making his effect, not whether he succeeded let alone whether his effect is good or bad' (p 92). (ii) Necessary standards, such as 'unity within variety', 'spontaneity', 'truth', 'sensuousness'. In treating something as a work of art rather than of entertainment, we consider something as a impact (p 123) hence considerations such as 'increase in range of sensuous and emotional response' (p 124), 'increase in capacity for happiness' (p 125), 'increase in subjective understanding of a greater range of character' (p 129). (iii) Hypothetical standards relate to the permanent and needs of the time and lead to judgments of the importance of works of art which may be 'out of all proportion to their value by necessary standards' (p 144). Thus for the present Kafka helps us to understand our situation: if readers of the future are baffled by our interest in this madman, so much the better for them' (p. 145).

In Part IV, the thesis about the non-descriptive use of language is extended to metaphysics, myth, religion, and morality. Graham the gram in writings on these subjects as the problem of verification. The gram in writings on these subjects as the problem of verification.

man, as if readers of the present Kafka helps us to understand the thesis about the non-descriptive use of language in art for the importance of works of the future are baffled by our interest in the proportion to their value by necessity. In Part IV, the problem genuine myth, religion, and mysticism. According to Graham the problem of how to separate the chaff from the grain in writings on these subjects. has been erroneously interpreted as the problem of verifying or falsifying descriptions about spiritual entities such as goodness, truth, beauty, the soul, God (p. 149) which are equally as real as if not more real than those described by the scientists. The bulk of Part IV is devoted to showing, with great skill, that when the problem is interpreted in this way, it becomes insoluble. For "the better is any other" (p. 150). However, when we realize that "scientists have understood the material world, the less reason we find to suppose there is not discovered a neutral universe, but learned to describe the universe in neutral language" (p. 153). we can acknowledge that the universe can and must be talked about also in language which is not neutral and that the problem of distinguishing between the good and the bad in such

committed talk must be solved by methods appropriate to it. All we need to do is "show that the standards by which we evaluate are as objective as the criteria by which scientists judge questions of fact" (p 155). "From the present point of view, criticism of a religion or philosophy of life is in the first place criticism of a way of feeling and acting, controlled and organized by emotive speech and gesture. I must choose my beliefs by the necessary and hypothetical standards which govern other kinds of choice" (p 157). Such religious or metaphysical world-pictures have no point of contact with the world-picture of science, except where they arouse in us certain factual expectations, such as that the adventures of pleasant people always have a happy ending, or that there will be in this world reward for the good and punishment for the wicked (p 157). Where a religious or metaphysical world-picture has such implications, it is subject to disproof by the sciences, and can often be shown to be false, as for instance "the religion of the older parts of the Old Testament" (p 157). If it promises rewards and punishments "in another world then the existence of heaven and hell becomes a question of fact, and we are back where we started, in the spiritual world which systematically eludes discovery" (p 157). The relevant kind of criticism of metaphysical and religious world pictures pruned of such implications capable of conflicting with the world picture of science, "is to object that the implied attitude to life is self-contradictory, or does not take into account the variety of human needs or is 'Nay-saying', 'on the side of Death', or gives an outlet in fantasy for desires which should lead to action, or has implications for action which conflict with moral or prudential standards" (p 158).

Who should read this book? The Introduction suggests that Mr Graham intended it for philosophical laymen, but the compression, the subtlety and complexity of many of the arguments, the things he takes for granted, would probably make it too difficult for the wholly uninitiated. However, I think it holds much for professional philosophers and for students, perhaps most for those who have not so far been especially interested in Ethics and Aesthetics, for it is bound to stimulate them to explore these fields further.

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*Our Experience of God* By H D LEWIS The Muirhead Library of Philosophy Allen and Unwin, 1959, Pp 301 Price 30s.

*Philosophy and Religion* By JOHN WILSON Oxford University Press, 1961 Pp viii + 119. 12s 6d.

*Christian Faith and Greek Philosophy* By A H ARMSTRONG and R A MARKUS Darton, Longman and Todd, 1960 Pp x + 162 15s

*Introduction to Religious Philosophy* By GEDDES MACGREGOR Macmillan & Co Ltd, 1960 Pp. xvi + 375 30s

If it is difficult to characterise Professor Lewis's approach in a few words, that is primarily because he rejects all the tempting and popular shortcuts to Christian theism. To believe in God is not, on his view, to affirm a purely moral commitment, nor is it to commend an arresting view or perspective of the world as we ordinarily experience it. It is to believe in a being who transcends the world, who is its irreducibly mysterious source

Despite the suggestion of these words, Lewis does not invoke the traditional cosmological argument to demonstrate God's existence. Rather, we are to meditate upon the various sorts of fragmentariness in the world, the "radical incompleteness of explanations", and by a single movement of the mind perceive that these witness to, even demand, a transcendent being, himself perfect and complete. To Lewis, this passage from the finite and dependent to the infinite and undervived provides the very spring and sustenance of religious awareness. It is not proof, though it is cognitive—it is not emotion, though it is charged with religious wonderment. The development of religion is essentially the articulation of this "insight", the perversions of religion are the betrayals of it. Although Lewis does not present a detailed Christian apologetic, he has many sane and illuminating things to say on topics like "Experience and Images" (some sharp discussion of Austin Farrer) the "Abuses of Dogma", and on "Art and Religion". He finds an easy passage from the strange and disturbing quality of some aesthetic experience to the religious sense of the world's contingency or dependence. The central claim of the book—its account of transcendence—is both impressive and most elusive to assess. Impressive, in that Lewis's many formulations of it express, and evoke in a sympathetic reader, experiences quite basic to any Christian theism. Elusive, in that for all the rejection of traditional cosmological arguments, Lewis still uses the vocabulary of those arguments—"dependent", "derivative", "complete", "unconditioned". He would reply that the movement of thought that carries us from the finite to the infinite is  *sui generis* . God does not "cause", "explain" or "complete" the world in any ordinary sense of these words, yet we have no other words that take us any nearer our mark.

Whether one accepts this account will depend on how keen or moribund is one's own religious experience, and on one's readiness or unreadiness to construe it cognitively, as Lewis, of course, does. But the very nature of his case precludes the rigorous demonstration of this cognitive status.

Like Professor Lewis, Mr. Wilson argues that the case for theism stands or falls by the reasonableness of religious beliefs, "facts" (in his words) "about the supernatural". But what kind of assertions are these statements of belief? They are not explanatory—or God runs the risk of being reduced to a thus-wordly, creaturely level. They are not "self-justifying"; nor adequately justified by appeal to authority. Wilson argues that they must somehow be backed up by experience. The verification-falsification challenge is entirely relevant to our claims about God. Such claims cannot be established on the basis of a single, even momentous, experience, but, like our claims about material objects, require a complex "network of actual or possible experiences". Given this, there would be no further, arduous problems over the cognitive status of religious experience or the objective reality of God. Existential statements being only a convenient shorthand for collections of experience-statements, the statement "God exists" would be a summary of actual and possible religious experiences. To deny this would be to subscribe to the misconceptions (a) "that there is a basic, ontological difference between what can be said to exist and what cannot", and (b) that "only perceptions . . . can be cognitive".

But the question remains: Do religious experiences have the patterns of recurrence and resemblance that would justify our gathering them up in the claim "God exists"? Wilson believes that only experimentation can answer this, and desires to see a programme by which "as many people as possible" might be led to have religious experience, and which would



provide more adequate and systematic data than we at present possess. Meantime, however, practical choice of religious commitment has to be made on less than ideal evidence. Rational choice is still possible here—by way of “an assessment of the genuineness and cogency of religious experience insofar as any particular religion is based on such experience, and of the validity of the structure which such experience has been made to bear”.

Wilson's little book is adroitly organized, and his arguments—particularly the destructive ones—are efficiently and economically deployed. I suspect that he underestimates the *logical* difficulties in construing religious experiences (no matter how prodigally they occurred) as experiences of a deity, described as Christian theism describes him. I also doubt whether any contrivable experiments could verify or falsify a claim like “God is love”. For any apparently contrary evidence, such as an experience of abandonment, could readily be taken as God's love manifesting itself in the disciplining and testing of fallen humanity. Conclusive falsification could surely never be forced.

Nevertheless, there is ample excuse for both Lewis and Wilson turning the spotlight back upon religious experience. If the germinal experiences are neglected and their peculiar quality forgotten, debate over the existence of God can easily lose sight of its original elusive focus and become a polemic over issues that the theist himself, in a “cool hour”, would not wish to defend. And in the last resort, the phenomena of religious experience and their *lisons* moral and aesthetic are of too great human importance to be ignored by philosophy, even supposing the case for traditional theism can never be exhibited as compulsive.

*Christian Faith and Greek Philosophy* is based on a series of extra-mural lectures given under the auspices of Liverpool University. It seeks to unfold what Christian theology adopted, borrowed, transformed, from Greek thought. Ten major topics are discussed, among them “God's Transcendence and Infinity”, “The Nature and Destiny of Man: Soul and Body”, “Love and the Will”, “Time, History, Eternity”. The book is short and the treatment necessarily sketchy, although well-packed with argument and allusion. Particularly effective is the chapter on “The Material Universe”—contrasting Stoic, Platonic, Gnostic and Christian attitudes to the natural world: and the discussion of *ἔπος* and *λόγος* is a useful corrective to the familiar over-dramatizing of the distinction.

In a rapid outline-study one cannot ask that all obscurities be made plain, but I should have liked to see some clearer admission of the appalling logical difficulties that would attend the analysis of statements like the following from Plotinus: “[the divine mind is] an organic living community of interpenetrating beings which are at once Forms and intelligences . . .”. The writers' own language sometimes slides between the philosophical, the theological and the devotional in a manner that can hardly help an “introductory” reader to order and discipline his own reflection. Thus: “it then becomes easy to see the Spirit as a further co-equal expression of the Father and the Son's union in love as their love itself, their delight and glory in each other.” “Easy”? Surely if one could ever be forced to say such category-defying things, it would be (should be) only with much perplexity and anguish.

Dr MacGregor's *Introduction to Religious Philosophy* reads like a lecture-course that tries to say something illuminating on almost all the major problems of philosophical theology. If one is teaching this subject, a choice has to be made either to attempt a “survey course” or to select for more

detailed and careful treatment a relatively small number of important topics. In either case there will be gaps: the survey must oversimplify, and the alternative approach must leave many problems not so much as mentioned. But the chief danger with the survey method is that students may never be initiated into the venture of genuine, tough, adult argument, with all its uncertainties and ambiguities. Instead, they tend to be subjected to a flow of processed, bland reflection.

"Bland", however, is not a fair epithet for parts of MacGregor's *Introduction*. On the problem of evil, for instance, there is a reasonably sustained and serious enquiry. But on the existence of God this cannot really be said. Arguments, MacGregor believes, cannot effectively demonstrate God's existence. "the only real proof . . . lies in the struggle with Him." What can be meant here by "proof" is hard to see; since the experiences described by a believer as "struggle" with God will be very differently interpreted by the sceptic. Problems of great complexity are in this section simply and blithely passed over.

The discussion of values is again too smooth and simple to be useful for a student at university level. For example. "The idea of the dignity of man is derived from and dependent upon belief in God." "What worth or dignity does man have in himself?" (expecting the answer, None). There is little enough argument offered for this view, and no serious consideration or adequate citation of arguments to the contrary.

This book is a strange and not very satisfactory mixture of the philosophical, autobiographical and anecdotal. Its style swings erratically between that of the pulpit and the lecture-room. Doubtless philosophy of religion has to be taught from some point of view, and it may do no harm for the point of view to be openly declared. Dr MacGregor's enthusiastic commendation of Christianity, however, comes a little too close to evangelization in the class-room, and one may have rather a different attitude to that.

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*Mysticism and Philosophy* By W. T. STACE. Philadelphia and New York: J. B. Lippincott Co., 1960. Pp 349 \$6

*The Teachings of the Mystics* Edited by W. T. STACE. New York: New American Library (Mentor Books), 1960. Pp 240. 50c (paperback)

"The publishers believe it is not too much to say that *Mysticism and Philosophy* will be recognised as a work of distinction and importance comparable to *The Varieties of Religious Experience* by William James." This is altogether too much to say. Yet Professor Stace's book is in many ways a good one. The questions he asks are the right ones, though he sometimes begs them instead of answering them (see, e.g. p. 36, on Zaehner). As a fairly large-scale philosophical examination of mysticism the book is to be welcomed. Still, it certainly has faults. He says in his preface "It is better to be vaguely right than precisely wrong." No doubt, but the best thing of all would be to be precisely right; and the worst, to be vaguely wrong, hovers uncomfortably near. Stace is surprisingly unscholarly in this book. He bases himself on a limited number of sources, and these tend to be secondary sources. (In one case, he makes a quotation of a quotation of a quotation from Plotinus, and the furthest back he goes in



important bits from his oral testimony. Again, no indication is given of how long after the event N.M. reported his experience, or of how long after the "experience" N.M. made his "interpretation" of it. Stace's standards of evidence are not high. He never considers, for instance, how far the translators of the passages on which he bases himself may themselves be "interpreting" the Hmdn, etc., writings in translating them.

Stace's view is perhaps no worse than the other extreme, seen in, say, Dom Cuthbert Butler's *Western Mysticism*, where it seems to be maintained not merely that only Christians can have "fully mystical experiences" but that only Catholics can.

It is not merely an interest in the phenomenology of religion that leads Stace to consider the Problem of the Universal Core. He considers it also for its connection with a sometimes suggested argument for the objectivity of mystical experience (the Argument from Unanimity). This brings us to the Problem of Objective Reference, the second of his two main questions. Is mystical experience objective "in the sense that it gives information about the nature of the world outside the human mind" (p. 134)? Stace's criterion for objectivity is order. "An experience is objective when it is orderly both in its internal and its external relations. An experience is subjective when it is disorderly either in its internal or its external relations" (p. 140). And what does he mean by "order"? "By order I mean law, that is to say, regularity of succession, repetition of pattern, 'constant conjunction' of specifiable items". In this sense, he claims, dreams and hallucinations are always disorderly.

He weakens his case somewhat by saying that he wants to define objectivity not in terms of "our particular world order" but in terms of "the general concept of order". This is generous of him, for it will let anything in, if the "laws" are allowed to be sufficiently complicated. "Order" is such a relative term that it needs to be fairly sharply restricted if it is to be of much use.

This order criterion Stace offers as an improvement on the more familiar public/private criterion. "An experience which is merely private is not objective, not because it is private but because, being private, it will always be found to be disorderly" (p. 143). But I find this criterion very hard to understand. Stace attempts to explain it by reference to the case of double vision. "It is not disorderly that a man whose eyes are crossed should see things double. But it is disorderly that the crossing of the eyes should produce the actual objective duplication of objects. For there is no law of nature under which this could be subsumed and explained." But who supposes that crossing of the eyes really makes things double? It is certainly true that there is no law of nature that covers cases where the crossing of the eyes makes things really double as opposed to merely making them look double, but this is hardly surprising, for there are no such cases. What actually happens in cases of double vision (and Stace himself admits this) does not run contrary to "law, that is to say, regularity of succession, repetition of pattern, 'constant conjunction' of specifiable items". Indeed, whatever actually happens in any given case we may confidently assume happens in accordance with "law, etc. . . .". What then is the value of this use of "orderly" and "disorderly"? Apparently it is meant merely to point the difference between things really being there or really being such-and-such and their only seeming to be there or only seeming to be such-and-such. But what positive advantage then do "orderly" and "disorderly" offer over more familiar terms like "public" or "objective" on the one side and "private" or "subjective" on the

other? I simply cannot see the force of Stace's apparent claim to have explained the notion of objectivity in terms of the notion of order.

Stace's own view is that mystical experience is not objective, but not subjective either: it exhibits neither order nor disorder; for there can only be order and disorder where there is multiplicity, but mystical experience is of an undifferentiated unity.

Later, and shorter, chapters are called "Pantheism, Dualism, and Monism", "Mysticism and Logic", "Mysticism and Language", "Mysticism and Immortality", and "Mysticism, Ethics, and Religion". The book is pleasantly written. Its faults are perhaps all the easier to see because Stace develops his points at length and, at least stylistically, with care.

The paperback *The Teachings of the Mystics* is presumably a by-product of the writing of *Mysticism and Philosophy*. It is a book of selections from "the great mystics and mystical writings of the world" (including Arthur Koestler)—Hindu, Buddhist, Christian, Islamic, etc.—interspersed with explanatory comments, and prefaced by a useful essay called "What is Mysticism?". The selection is a sensible one. Hinduism is represented not only by the Upanishads but by Sri Aurobindo, though there seems no good reason why Stace should have taken his selections from the first three, largely introductory, chapters of *The Life Divine* rather than from nearer the end—say, from the chapter called "The Evolution of the Spiritual Man" onwards—which would have represented Sri Aurobindo's views on mysticism more adequately. On Zen we have (of course) Suzuki, but also Herrigel. The selections from Christian mystics are well chosen, though it is a pity he did not include something from the English mystical classics, especially the subtle *Cloud of Unknowing*. Stace implies that Dionysius the Areopagite by-passes the "problem of evil": he may not discuss it to much effect but he certainly discusses it.

THOMAS MCPHERSON

*Foresight and Understanding: An Enquiry into the Aims of Science* By  
STEPHEN TOULMIN. London: Hutchinson and Co. Ltd. 1961. Pp  
115 18s.

IN this little book, which is based on a series of lectures delivered at Indiana University during 1960, Professor Toulmin aims to show us something of 'the fascinating problems that arise when one brings logical and philosophical questions to bear on the history of our scientific ideas' (94). To this task he comes equipped not only with formidable talents as a philosopher and historian of science but also with an obvious enthusiasm for his subject. Little wonder, then, that his case—presented with characteristic vigour and lucidity—is an impressive one: indeed, its main defects are due to its brevity; one would like to know more about his answers to these 'fascinating problems'.

The central question for discussion is: 'What gives scientific ideas merit, and how do they score over their rivals?' Toulmin sets out, furnished with an apt selection of historical examples, to refute a current philosophers' answer—that the merits of a scientific theory are proportional to the predictions or forecasts which they imply—and ends by recommending an evolutionists' one—the question itself, he claims, is equivalent to the Darwinian formula: 'What gives them survival-value?' (111). The problem of 'scientific merit' turns out to be 'the

problem of seeing in how many ways a novel scientific idea may, in the conditions of its introduction, be "better adapted" than its predecessors or rivals' (17)

His argument is presented straightforwardly in three main phases: (i) explanatory power is not equivalent to predictive success, there is a difference between forecasting technique and genuine understanding (chap 2); (ii) we achieve understanding of the observed regularities of nature only by relating them to certain explanatory paradigms or 'ideals of natural order' (chaps 3 and 4), (iii) these forms of explanation survive and evolve, not just by meeting some single demand such as conformity with the facts, but in so far as they meet the multiple demands of their social and intellectual environments (chaps 5 and 6). The value of the historical material which Toulmin uses to carry this argument along is not (I imagine) likely to be disputed. But some of the philosophical points he raises are more contentious. At least they call for comment.

(i) *Prediction and explanation.* Toulmin begins his dialectical examination of some prevalent myths by disposing of the idea that science has one purpose only—to explain. This thesis, he argues, equivocates between falsity (therapeutic and classificatory sciences are counter-examples) and tautology (the thesis is commonly rescued by defining as 'scientific' those activities only whose task is to explain). At this point the predictivist thesis, 'A successful explanation is one that yields many predictions', promises to rescue us from the second horn of this dilemma without throwing us back on the first. Indeed, the thesis has several attractions: it appears to give a genuinely informative and illuminating account of explanation, it offers us 'a simple, sure and even quantifiable test for choosing between good theories and bad ones' (24); and it avoids all philosophically troublesome reference to natural necessity and counterfactual conditionals. Against this, however, Toulmin once again employs Hume's fork to good effect. For on a naive interpretation (where 'prediction' means 'categorical forecast') the predictivist thesis is false; while on any interpretation which does justice to the facts the idea of explanation is taken for granted, not defined and the old philosophical troubles crop up again. We are now back to where we started and therefore presumably, more receptive to the account which Toulmin himself is to give. But notice, however, that we are back also with the problems of natural necessitation and counterfactuals. To be sure, Toulmin tries to throw some light on the former: a nexus of natural necessity, he suggests, is a 'mechanism' of some sort (36), or an 'ideal of natural order' (38) which accounts for our predictive success, and this does something, though not much, to quieten our uneasiness about natural necessity. But about counterfactuals he has nothing whatever to say: the problem is not even stated let alone solved by the bare reference to ideal of natural order.

(ii) *Ideals of Natural Order.* On Toulmin's view of the matter scientific explanation consists of relating phenomena, i.e. events whose causes are in question, to certain 'principles of regularity, conceptions of natural order paradigms, ideals or what you will, intellectual patterns which define the range of things we can accept (in Copernicus' phrase) as "intrinsically absolute and pleasing to the mind"'. He continues: 'Any explanation, to be acceptable, must demonstrate that the happenings in question are special cases or complex combinations of one or more fundamental intelligible types'. (81) Now, although Toulmin does not give examples of these explanatory paradigms from the field of natural science,

(chap. 3) and chemistry (chap. 4), there is still a good deal of looseness in the idea. He tells us something about their natural history by illustrating the ways in which such paradigms have established themselves and altered in the course of our intellectual development. And he tells us something about their psychological status: they are 'absolute . . . and pleasing to the mind' and operate for most scientists as preconceptions which they bring to rather than derive from the world. All this is very interesting and valuable. But he does not tell us enough about their *logical* status. To be sure, he compares them with Collingwood's 'absolute presuppositions' (16) and suggests that they 'are not "true" or "false", in any naive sense' (57). But are they true or false at all? Or does this hint at a reversion to the rule-interpretation of natural laws found in Ramsey, Schlick and the earlier Toulmin? Further, we want to know more precisely what is to count as 'an ideal of natural order'. Any universal generalisation whatever? Any idea that any men happen to accept as paradigmatic in their attempts to explain Nature? The only requirements he states—and his examples do not 'show' any more—are that they should be 'consistent with the numerical records', 'absolute' and 'pleasing to the mind'. Sun-gods and tautologies (faculty-explanations) alike would seem to satisfy these. Again, it is unclear as to how general a principle of regularity must be before it counts as an explanatory paradigm. Some of the examples are of *kinds* of explanations, e.g. the 'material principles' of eighteenth-century chemistry (89). But can a *kind* of explanation be itself an explanation? Other examples are particular instances of highly general laws, e.g. Newton's second law of motion (57). These may provide explanations but they are not always—even for their propounders—'self-explanatory' or 'absolute . . . and pleasing to the mind'. Clearly, the notion of 'ideals of natural order' needs a lot of tightening up.

(ii) *Evolution of Scientific Ideas.* Toulmin's argument culminates in his thesis that what gives scientific ideas merit is identical with what gives them survival-value, viz. their ancestry and environment. He takes this evolutionary talk very seriously. The thesis is worked out in terms of such analogues of Darwinian biology as: natural selection—'change results from the selective perpetuation of variants' (110); chance variation—'an inheritable variation sometimes appears in a population first by chance, conferring at that time no particular advantage on its possessors: yet this same variation may subsequently become of extreme value to their descendants as a result of changes in the environment' (113); struggle for survival—'For every variant which finds favour and displaces its predecessors, many more are rejected as unsatisfactory' (111); and so on. As a way of looking at the development of scientific ideas, this can be very illuminating; and Toulmin makes it so. It is certainly a rewarding analogy. But is it any more than that? It seems that if we take it too seriously we get into difficulties. Suppose, for instance, that through some quirk of history, men were again to accept as paradigmatic in their explanations of natural processes universal principles which were tautologous in form, e.g. so-called 'faculty-explanations' or 'material principles'. Would Toulmin still want to say that survival-value is the criterion of scientific merit? Like so many biological evolutionists, he seems to presuppose, what is highly debatable, that 'evolutionary development' is synonymous with 'progress': that the species or idea which survives is better than or has more 'merit' than its predecessors. But it is just this notion of merit, in its application to scientific ideas, that he is trying to

explain. The problem can be put another way by asking 'What is the logical status of Toulmin's thesis that the best explanation is that which survives?' (Compare 'What is the logical status of Darwin's thesis that the fittest species is that which survives?') We want to know whether Toulmin has a criterion of scientific merit *independent* of the notion of survival value. For if not, his thesis is tautologous. While if he has, it is surely a mistake not to tell us what it is. As it is we are left free to imagine circumstances in which the magical explanatory paradigms of Australian aborigines might have to be rated as more meritorious (better explanations?) than those of 20th-century physicists just because they alone have survived. Or has Toulmin *a priori* grounds for dismissing the fears of Trafalgar Square sitters?

These comments, if fair, will warrant the following conclusion. Toulmin is at his best, in this book, when he is confronting philosophical theories with the facts of our intellectual history; but he is much less secure when he sets out himself to account for the nature of scientific explanations and to give the proper criteria for their evaluation. This is not to say that his account could not be made to work, but only that he does not make it work here.

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R D BRADLEY

*Rightness and Goodness: A Study in Contemporary Ethical Theory* By OLIVER A JOHNSON. Martinus Nijhoff, The Hague. 1959. (International Scholars Forum.) Pp viii + 163. Price not stated.

MR JOHNSON takes us back to the 1930's, both in his subject and in his method of approach. His book is a critical discussion of the deontologists (Priest, Ross, and Carrick), and ends with a positive theory grounding rightness on goodness. The deontologists argued that if the rightness of an action depended on goodness, the goodness would need to be either in the consequences or in the motive. They held, Johnson points out, that good consequences are a necessary, but not a sufficient, condition of rightness, while a good motive is neither a necessary nor a sufficient condition. Against them, Johnson holds that good consequences are not even a necessary condition of rightness, while a good motive is a necessary but not a sufficient condition. The sufficient condition is a good motive together with either good consequences or 'organic goodness'. The non-utilitarian duties of promise-keeping, justice, reparation, and gratitude depend on organic goodness. An action or situation is organically good if it contributes to a good way of life.

To support his contention that good consequences are not a necessary condition of rightness, Johnson has two arguments, both relating to promises. In the first place, he accepts the view of the deontologists that the obligation to keep a promise arises simply from the making of the promise, and he argues that this is inconsistent with the concession they make to utilitarianism that an action cannot be right unless it is thought to produce some good. Secondly, he constructs an imaginary case of a private promise to a dying man to have his body cremated instead of buried. If the person who has promised does not believe that the soul survives death, the fulfilment of his promise will produce no good consequence; yet it is a strict obligation.

I think that both these arguments are fallacious. As regards the first,



Johnson overlooks the fact that the thought of good to be produced is built into the concept of promising. A promise is an undertaking to do something for someone, and this means something that the someone regards as a benefit. The word 'promise' may be used ironically of a threat ('I shall give you a good hiding, I promise you'), but neither the issuer nor the recipient of such a 'promise' would think that failure to carry out the threat constituted a breach of obligation. The second argument relates to a situation where the recipient of the promise can no longer be benefited. But does Johnson seriously think that cremation produces no good consequences at all? It at least prevents the bad consequences of leaving a dead body to rot exposed. Of course the man who has made the promise may not think that cremation is any *better* than burial, but the principle which Johnson is criticizing is that a right action must produce some good consequences, not that it must produce the best possible consequences.

Johnson's presentation of his second thesis, that a good motive is a necessary condition of rightness, is rather more complicated. He criticizes, with care and with some cogency, Ross's two arguments, (a) that moral motives are not within our control, and (b) that a duty to act from a sense of duty involves an infinite regress. Having established that there *can* be a duty to act from the sense of duty, Johnson then argues that there *must* be such a duty; for the moral motive displays the highest kind of moral character, and we all have a duty to be morally good. The premisses as stated do not show that *every* duty or right action is a duty to act from the sense of duty, but Johnson does make a strong case against Ross in this part of his book. When discussing a connected issue in the theory of the deontologists, he brings out a difficulty which seems to have escaped their notice. So long as they held the objective theory of duty (*viz.* the theory that a man's duty is to bring about a result which is objectively right), they could also hold that a man might do his duty from a moral, or a non-moral, or an immoral motive. But when Prichard and Ross changed their ground and adopted a subjective theory of duty (*viz.* that a man's duty is to do what he thinks right), it was no longer easy to say that a man might do his duty without acting from a moral motive. For, Johnson urges, it is psychologically impossible both to be convinced that an action is right and to do the action without being influenced by that conviction. The deontologists could still say, no doubt, that the agent might act from mixed motives, being influenced at least as much by non-moral (or even immoral) considerations as well.

The doctrine of 'organic goodness', according to which the rightness of non-utilitarian duties depends on their exemplifying a good way of life, is a modification of the theory put forward by H. W. B. Joseph. Johnson argues that his view is superior to Joseph's, because Joseph's good 'form of life' was that of a community and so merely hypothetical, while Johnson's can be exemplified in the life of a single individual and so be actual.

Johnson's criticisms of the deontologists are of uneven quality. This is how he deals with Prichard's principle that an 'ought' can only be derived from another 'ought'. Prichard says: 'The word "ought" refers to actions and actions alone. The proper language is never "So and so ought to be", but "I ought to do so and so".' Johnson objects that the first sentence of this quotation is false, because 'ought' can be properly *predicated* only of persons, not of actions; 'right' can be predicated of actions, and 'ought' can be derived from 'right', since the statement that an action is right implies that one ought to do it; Prichard

(Johnson thinks) accepts this implication, and so contradicts his principle. Johnson's argument here is singularly ineffective. In fact Prichard himself insists that obligation is a predicate (or 'attribute') of persons, and holds that the obligation to do an action *cannot* be derived from the not yet existing character of the not yet existing action. Ross's views are discussed at greater length, and with greater care, as I have already indicated. Johnson oddly supposes, however, that Ross's account of rightness as suitability, is an alternative to, and inconsistent with, the 'putative' or doubly subjective theory of duty. Both views are given in *Foundations of Ethics*, where Ross holds, (a) that rightness is suitability to a situation, and (b) that a man's duty is to do what he thinks right, *i.e.* what he thinks suitable to what he takes to be the situation. These two theses are independent of each other, and there is no inconsistency in holding both.

Despite such lapses, I hope that the earlier part of this review has shown that Johnson's book contains some fairly nourishing meat for those who still have an appetite for the problems of old-fashioned moral philosophy.

D. D. RAPHAEL

*Perception and The Physical World.* By D. M. ARMSTRONG. Routledge and Kegan Paul, 1961. (International Library of Philosophy and Scientific Method.) Pp. xii + 196. 25s.

DR. ARMSTRONG announces in his 'Introduction': "... this book is a defence of Direct Realism. . . . But . . . I hope also to ask and answer the questions 'What is perception?' 'What is a sense-impression?' and even to cast some indirect light on the question 'What is a physical object?'" Although Armstrong presents his account as an answer to the question "What is the *direct or immediate* object of awareness when we perceive?" (p. xii: *italics in original*). Direct Realism is best understood indirectly. What it is is: neither any sort of Phenomenalism; nor any variety of Representative view. It is thus appropriate that he should begin, as he does, by trying to dispose definitively of every possible variation on these two themes.

His tactic is to introduce them as alternative responses to arguments from illusion. Macbeth hallucinated does not, of course, see a real dagger. However, he does—supposedly—see something, the subjective simulacrum of a dagger; "and if it is admitted that the very same experience could be a veridical perception, it can hardly be denied that even in veridical perception the *immediate* object of perception is a sense-impression" (p. 27: *italics in original*). Armstrong deals fully, fairly, and very faithfully with the consequences of this conclusion: and thereby becomes "sufficiently emboldened" (p. 80) to proceed to his own analysis of sensory illusion.

The conclusion is that a sensory illusion just *is* a false belief: "when (or in so far as) we suffer from sensory illusion there is no object at all, physical or non-physical, which we are perceiving in any possible sense of the word 'perceiving'. There is simply the completely false belief that ordinary perceiving is taking place" (p. 83). This is a fine bold thesis, and one which—whether right or wrong—it is a solid contribution to have developed with such sincerity and power. But if justice is to be done it is essential to separate this contention very sharply from its phenomenological analogue. It is one thing to urge that undergoing an hallucination is not a sort of perceiving, that 'seeing' is not any kind of seeing. It is quite



the place of the old International Library of Psychology, Philosophy and Scientific Method edited by C. K. Ogden. The old sober green cover is replaced by a lively red, the paper is tougher and whiter, the print clearer. The new Library deserves a warm welcome.

This particular volume aims to give a history of the philosophy of perception in just over 200 pages, a formidable undertaking. Mr. Hamlyn proceeds by devoting a chapter to each of the main historical periods, and gives an account of the views on perception of the chief philosophers in each period. Two difficulties face any such undertaking. First, the 'problems' of perception discussed in one age are often very different from those discussed in another so that continuity of theme is difficult to achieve. Secondly, in some philosophers, such as Spinoza, the discussions of perception have to be wrenched out of a philosophical system in which they are embedded. The author is not entirely successful in overcoming these difficulties: and perhaps no one could be. Ancient and Mediaeval Philosophy present the most difficult problem. The first chapter, after a few pages on the Presocratics and Plato, gives a rather more extended account of Aristotle on perception. But it is hard going and would tax any one but the specialist. The second chapter on Hellenistic Philosophy deals with Epicurus, Stoicism and Neo-Platonism. In giving only four pages on Stoicism and only a few lines on the New Academy, Mr. Hamlyn seems to have missed an opportunity. The Stoic insistence that perception can give knowledge, in their doctrine of the *καταληπτικὴ φαντασία*, and the attack on this doctrine by Arcesilaus, Carneades and Chrysostomus of the New Academy, with its appeal to illusions, hallucinations, mistaken identity, etc., has more in common with modern discussions of perception than most ancient philosophy. The account of the controversy between the two schools and the final compromise position taken up by Philo of Larissa before the Academy capitulated to Stoicism under Antiochus of Ascalon would have formed a good introduction to modern discussions. Although Cicero's *Academics* gives only the bones of this controversy, it has been well reconstructed, notably by V. Brochard in *Les Sceptiques Grecs* (a book which would have deserved inclusion in Mr. Hamlyn's useful seven pages of Bibliography).

Chapter 3 on Mediaeval Thought gives brief accounts of Augustine, Aquinas, Bonaventure and William of Ockham. Chapter 4 is an introduction to the seventeenth-century preoccupation with method; and Chapter 5 deals with Descartes, Malebranche, Spinoza and Leibniz. Up to this point the reader must have found things rather hard going. But with Chapter 6 on the Empiricists the story becomes more coherent, because perception and sensation now form a main philosophical theme with Locke's 'new way of ideas'. Henceforward we get interesting chapters dealing with the idealist reaction to empiricism (ch. 7), nineteenth-century sensationalism (ch. 8), the reaction against this (ch. 9) and the sense-datum and phenomenological theories of the twentieth century (ch. 10). This is straightforward history with comments occasionally from the author of which the following may serve as a specimen.—It is a great error of the causal theory of perception that it runs together the concepts of sensation and perception (p. 101). It is a merit of Thomas Reid to have distinguished the two. He does so by maintaining that when we use our senses both processes—sensation and perception—occur simultaneously. This raises a problem which is better solved by saying that what happens can be viewed in two different ways, as sensation or as perception (p. 123). The section on Reid is an interesting and valuable part of the book.

In the final chapter Mr. Hamlyn draws conclusions from the history and makes some suggestions of his own. Here they are, briefly summarised as best I can. (1) Some philosophers have assimilated perception to sensation, others to judgment. (2) Reid and Bergson are almost alone in clearly distinguishing sensation from perception. (3) In modern times Ryle's denial of visual sensations and restriction of the term *sensation* to feelings of pain and touch experiences, is rejected on the grounds that "our eyes furnish experiences which could be called sensations" (p. 191). (It would be better, I think, to say that they are like sensations in that people *have* them in the same sort of way, i.e. they are aware of them but not by observation.) (4) Sensation is a necessary condition of perception: perception is a sufficient condition of sensation. (5) Having a sensation is not a *part* of perceiving. (6) In the primary sense of 'see' (seeing being a variety of perceiving) to see something involves identifying it. (7) Perception is not something that happens to a man nor something he does. Like the concept of knowledge that of perception presupposes the notion of a standard. (8) The notion of a standard could only arise in society.—These indications of a systematic account of perception are necessarily brief and it is to be hoped that Mr. Hamlyn will develop them at greater length. Meanwhile he has written a history which, in spite of the formidable obstacles confronting him, is likely to interest all concerned with perception.

I have noticed a few misprints: on p. 31 in the title to chapter 2 *Hellenistic* should be *Hellenistic*: p. 49, 1.12 *no* should apparently read *nowhere* p. 201, the translator of Hegel's *Phenomenology of Mind* is *Baillie*, not *Bailey*: p. 203, the name should be P. F. Strawson, not C. F. Strawson.

W. H. F. BARNES

*Scientism and Values* Edited by HELMUT SCHOECK and JAMES W. WIGGINS D. Van Nostrand Co., Princeton, New Jersey. Pp. xvi + 270. 49s.

ARE the standards of the natural sciences always appropriate to the social sciences? Might not economics, psychology, and sociology—studies, that is, of a quite different sort from physics and chemistry—require explanatory models of a quite different sort? These are the interesting, though banal, questions raised in this collection of papers. Raised but not settled, for early in the book it becomes clear that the contributors are less explorers in this ill-charted region than troops already on the march. Their banner reads "Value"; the enemy's "Scientism" (or sometimes "Neopositivism", "Reductionism", "Behaviourism"). Supporters of "Value" believe that to describe man and societies adequately one must use evaluative terms, and that such terms are irreducible. Supporters of "Scientism" believe that the techniques of the natural sciences are the only ones permissible for an intellectually adequate study; hence they apply techniques which are appropriate in some places in places where they are not appropriate. The contributors all agree that they are, and not just that they might be, inappropriate in social studies. The source of this confidence, though, is unclear. Eliseo Vivas, in one of the better papers, sets out to "show that the disciplines fashionably called at present

'the behavioural sciences' are not scientific in the sense that the physical and the biological sciences are scientific" (p. 50). He does not *show* it, though he does effectively ask why some people so piously think "behavioural sciences" should be scientific. Pieter Geyl, in another of the better papers, sets out to show that history cannot be made scientific; he too fails, but he cites with effect some of the rubbish written by those who think it can. I do not mean these remarks about Vivas and Geyl as criticism of the book, for I believe that the aim of most of the contributors is not discussion but battle; their pieces are intentionally polemic not proof.

As polemic the book seems to me to fail in two ways. Some shafts, without doubt, hit their mark. Murray Rothbard examines how economics borrows from natural sciences both vocabulary (*e.g.* "equilibrium", "elasticity", "friction") and techniques (*e.g.* the reduction of explanation to functional formulae). He does convince us that there is a lot of slavish imitation in the social sciences; but what he does not anywhere do is show that the borrowing involves some sort of mistake, that the terms and techniques do not fit the subject. Ralph Lewis, in an interesting explanation of how "growth" appears in biology and, derivatively, in theories of education, accuses some psychologists of going to the extreme of using biological facts as sign of parallel psychological ones: biologists speak of "pace" in growth of organisms, so the search begins for "pace" in intellectual growth. Lewis succeeds in making us feel the strain and artificiality of this whole programme; but nowhere does he tell us whether, or why, or to what extent the attempt to introduce measurement and experimental procedures into psychology is wrong. In fact, nowhere in the whole volume do the authors define scientism and their objections to it much more specifically than I have done here. Only one contributor, Robert Strausz-Hupé, hints at greater complexity. He allows (p. 222) that the methods of natural science may not be *wholly* out of place in social sciences. But, alas, this is only in passing. He does not pause to tell us where then they might be appropriate, nor to give even one illustration of their inappropriateness. Thus, I think, is the first failing of the polemic. The banners under which the authors march bear such simple mottoes that one begins to wonder not what the struggle is but whether, were the authors more specific, there would be a struggle at all. With more detail, one feels, the authors might have the demoralizing experience of seeing a battalion or two of "neopositivists" come over to their side. As it is, on the level of generality they prefer, who would oppose them but an army equally content to fight for unexamined slogans? For both these reasons, their struggle loses our concern; and polemic, whatever else it can be, has at least to be interesting.

The attack takes on the aspect of a Holy War when it shifts from scientism to what we are continually assured are its political views. Scientism "leads to" social planning, Ludwig von Bertalanffy says, presumably because if one believes that society has laws, and that men can discover them, then one will believe that men should control society. "Human engineering" will bloom (p. 206); "parliamentarianism" will wither (p. 252); and (the aesthetic argument against the political left) society will lose "those unique and imponderable factors—nuances, if you will—which endow life with zest, flavor, and creativity" (p. 222). It is no accident, we are reminded (p. 222), that several "modern dictatorships have sought to derive the warrant for their authority from the alleged precepts of science". (To be assured of the pervasiveness of these views consult also pp. xiii, xiv, 20, 44, 45, 83, 122, 127, 131, 136, 139, 176, 223-4,

247, 256) In short, (1) scientism leads left, (2) which is bad. This is the second failing of the polemic, it demands for its effect that one swallow both assertions. One's taste for (2) aside, (1) should stick in any gullet. If (1) means that there is something in the beliefs of scientism that requires one to be politically left, I disagree; there is no inconsistency in believing that there are laws of society, knowledge of which enable one to control it, yet in believing that such control should not be exercised. If (1) means that those of scientistic bias generally also lean left, and those without the bias do not lean left, my experience makes me sceptical. Perhaps the confidence of many social scientists in scientific procedure is non-rational, perhaps political beliefs are too. But looking round one, does it not appear that the well-springs of these two feelings are sufficiently separate for them to bubble to the surface of the human psyche independently?

JAMES GRIFFIN

*Ethical Naturalism and the Modern World-View.* By E. M. ADAMS.  
North Carolina University Press (London · Oxford University Press)  
1961 Pp. xii + 129 42s

In this book, Professor Adams is concerned to inspect three different forms of ethical naturalism (which he thinks covers ethical naturalism as a whole) to see if any of them will do; he concludes that none will and that as a result "there are objective values in reality independent of our experience of them". Adams claims to be at least mainly interested in analysing our moral discourse, he has been greatly influenced by English philosophers and quotes widely from them. Quite how the "Modern World-View" part of the title is matched by something in the text I am not sure, I think it is that Adams looks upon the temper of our age as being predominantly naturalistic and that therefore we suffer an unfortunate bias in favour of naturalism.

All of which is good enough. But unfortunately the book is extremely uneven in two different ways. In the first place, although Adams says frequently that his aim is "a careful consideration of how we use moral language in ordinary situations" (p. 130), in practice we often find ourselves asked to consider such questions as, "What . . . can the desire itself be if not in some way a cognition or an awareness that the factual validity-conditions normatively demand or prescriptively require the thing presented?" (p. 168). Even when he really does try to come to grips with idiom he keeps constructing artificial equivalences which vitiate his enquiry, e.g. he tells us that "whenever one says 'I like a' one might say 'a is what I want'" (and he clearly thinks these equivalent) (p. 136) which is surely just blatantly false. There are also lapses into rhetoric which become more frequent towards the end of the book (cf. particularly p. 186). Secondly, the book is uneven in philosophical quality, and the 'slips' which result are not unimportant. For instance the refutation of the Emotive Theory offered (last half chapter III) depends upon 'valid', 'rational' and so on being descriptive and not evaluative in ethical contexts. Not only would reading Urmson's 'Some Questions Concerning Validity' presumably have changed his mind on this matter but careful consideration of his own later statement that "'irrational' and its family are condemnatory words, whereas 'rational' and its cognates are commendatory" (p. 139) should at least have made him look more closely

All this is a pity because the book is based on two ideas which are of considerable contemporary interest. In the first place, Adams tells us that he started his work "with the conviction that some form or other of ethical naturalism could be made tenable" but that ten years with the problem, the product of which is this book, led him to change his mind. As there are other philosophers today (certainly round London) who have a similar conviction as regards some form of naturalism, a study, really well done, by one who has run the course himself could not help but be of interest. Secondly, he suggests that his interest is to decide the question of whether ethical language "is reducible to some other [type of language or language category] in the sense that whatever is sayable in terms of it can be said in terms of another" (p. 23). This is an aspect of the problem which Professor Hampshire and others have been concerned with, namely, what concepts and what categories are necessary parts of language. It is a fascinating problem but as yet a great deal of work has to be done just to formulate it. In the standard use of 'say', in order to say, "He is a good man", we have no alternative but to say, "He is a good man". What Adams needs is a well defined philosophical sense to be given to "say the same thing in different words". This is the old problem of what it is to produce an analysis in the tight sense of that word, but because the problem's old it's not gone and if we are to talk in terms of "whatever is sayable in one sort of language is sayable in another", then it must be tackled. Adams makes no attempt to do this; in fact, he shows no awareness that there could be a problem at this juncture.

In a sense I have painted too gloomy a picture of this book, for there are some good bits in it, e.g. the extended discussion of the Naturalistic Fallacy in chapter II. But good bits do not make a good book; taken as a whole its uneven nature ruins it and completely vitiates the general conclusions. It is well produced with clear type and wide margins; there is a reasonable index. But then, at roughly 2½ a page, that is no more than one has a right to expect.

JOHN WHEATLEY

*Edmund Husserl's Ethische Untersuchungen Dargestellt anhand seiner Vorlesungsmanuskripte.* By ALOIS ROTH. Den Haag: Nijhoff. 1960 (Phaenomenologica 7) Guilders 15

DURING his life-time Husserl published nothing on ethics. Now Roth has been doing researches on his unpublished manuscripts of ethics, lectures which he gave at the University of Goettingen intermittently between 1889 and 1924. The result of this work deserves, at least in my view, admiration. Roth has succeeded in constructing a consistent theory of ethics from the bits and pieces of Husserl's notes. Husserl began to construct his ethics by discussing critically several historical types of moral philosophy. Beginning with the detection of a self-contradictory confluence of means and ends in Hedonism which could be cleared, not as a conceptual mistake, but as an unavoidable fallacy of reason in the shape of Scepticism, he traces Hume's moral philosophy at some length to prove that a psychological foundation of moral behaviour would be completely insufficient. On the reverse the Rationalistic approach to 'morality' would end, as could be proved in the cases of Cudworth and Clarke, in erroneously combining the temporal and the eternal order. But, Husserl



grants, the Rationalists of the Cambridge Platonist school rightly hold the idea of an innate activity of the human consciousness against empiricism. Thomas Hobbes's foundation of the social order in basically egoistic desires, although standing on the opposite side of understanding morality rudimentarily contains the idea of grounding ethics in an *a priori* subjectivity. In fact, Hobbes, whom Husserl criticizes sympathetically, was an Idealist under the cover of his Mechanism. Kant's foundation of the categorical imperative in pure Will simply ignores the intentional life of consciousness, which clearly shows an activity from an inward "*a priori* feeling" towards moral values, as Husserl believes. Kant's categorical imperative involves self-contradiction: he takes refuge in an affection on the side of the empirical consciousness to bring it to respect the purely formal law. Kant, as Scheler alleged, would have deadlocked ethics in 'formalism'. Even the concept of Will is, Husserl observes, conceived like a law of nature which is not linked internally with the flux of our temporal life, the resource of all our moral activity.

Yet the form of a 'categorical imperative' persists in Husserl's construction, indicating its apodictic cogency in any defensible theory of ethics by necessity of the problem. Ethics should be free from any kind of "Weltanschauung". Following Brentano, Husserl observes a strictly systematic approach to ethics. The *Logische Untersuchungen*, which have resulted in a foundation of logic on the one side and in a groundwork of 'logical evidences' on the other, would suggest an ontology of values in 'paradigmatic' relation to the logical order. The second half of the book gives a systematic climbing into the formal and material structure of values, and investigates the mode of apprehending it. Husserl believes in the existence of a pyramid of values ("Wertekosmos"), ranging from the lower sensuous to the higher qualified intellectual values and finally to the "summum bonum formaliter spectatum".

Conceptually it would be accessible by a formal axiology ("formale Axiologie") which is materialized, if I understand Husserl correctly, in an axiology of contents ("Materiale Axiologie"). The formal axiology offers a number of propositions in which the order of the "Wertekosmos" is logically evident. All those propositions are built up in analogy with the three principles of Aristotelian logic. They show with evidence that the "Wertekosmos" contains primary, derived, and composite items of values. But, distinguished from the 'Organon' of logic, the "Wertekosmos" reveals a more flexible structure. Apart from the effect which the possible shifting within the "Wertekosmos" has upon the validity of each item of values, resulting from its being constituted by maximal, minimal and neutral, or absence of, values, there are certain modes of compositions and overlappings of values possible in principle. This would prove, as I understand it, that the entire "Wertekosmos" is subject to alterations which a pattern of logical evidences never could be. The "Wertekosmos" reveals an ontological, and not a logical structure. It only borrows the rigidity of logical evidence, in the light of which Husserl strove after, constituting and differentiating between the principles of ethics and those of logic.

The existing order of values corresponds to our valuing activities. Each individual apprehends and 'verifies' the order of values from his own position in a given situation in time. This leads Husserl to inquire into the nature of apprehending values (chapter, "Wert und Werterfassen"). Here, those, by now well-known, notions of "motivation", of an individual's inward "disposition" to act, of "empathy", in short: all the

elements of this 'microcosm' of human modes of consciousness have their place and constitute the criteria of apprehending values appropriately to the "Wertekosmos" outside. Husserl, as Scheler a few years later thinks of an "*a priori* feeling of material values" in a way that our intentional life aims for the fulfilment of values of which we are aware potentially within ourselves. The Wertekosmos gives 'direction' ("Richtung") to human actions in situations. Now Husserl can think of a 'categorical imperative' which must be so formulated as to render all human activities, the theoretical and artistic ones included, subject to this order. Following Kant's scheme of approaching the categorical form of the imperative by means of a preceding 'hypothetical' version of it, Husserl finally suggests the following, categorical rules "Always do your best out of that which you possibly could achieve in the whole of the sphere that is subject to your influence by reasoning" ("Tue jederzeit das Beste unter dem Erreichbaren in der gesamten, deiner vernünftigen Einwirkung unterworfenen Sphäre").

In challenge and response to the temporal environment individuals will form their habitual dispositions, and finally their characters, in view of the order of values that is imposed upon their thought. If I understand Husserl properly the individual's struggle between challenges and responses, in the light of that command from the "Wertekosmos", should lead to our adapting those values during our time on earth. This striving for the best that could be achieved by each person in a situation of his own, indicates an underlying, universal and teleological order which should give meaning to all the enterprises of mankind in history. Husserl makes this imperative an end for men, an end which should mature into this teleology. To incorporate and to live with all the splendid spiritual inheritance of our (christian-humanistic) civilisation. I do not deny that Husserl's work seems to progress in Ethics beyond Rationalism, Empiricism, Utilitarianism and finally beyond Kant, and I would say that the imperatives which result from the "Wertekosmos" appear to aim at the realisation of the ideal of the perfectly good personality in its practical conduct of life. A more careful study of the book might show obvious parallels not only with Brentano, Scheler, Hartmann and von Rintelen, but also with Moore's detection of the "naturalistic fallacy". It has become clear to me that Husserl wants to bring the genetic side of the personality into the validity of moral rules. According to this theory the history of the individual is contemporaneously the forming of its final disposition to future actions under the command of those values. Rightly, I think, the author comments that this ontology of values would become fully lucid only in the adumbrations of the existential-ontological belief in the 'radical finitude' of man.

But is this "Wertekosmos" as defensible after a critical examination as it appears to be if one judges Husserl only from his power to construct an ethics? Does this "Wertekosmos" really fit into the nature of consciousness as Husserl and Scheler would have us believe? Are we able to perceive, understand and incorporate the order of values appropriately?

First of all Husserl does not give any explanation of the origin of the "Wertekosmos". Here the reader is referred to his later conversion of phenomenology into an idealistic '*prima philosophia*' (in which I think it could occupy a place, although a less prominent one than it appears to hold in the ethics alone). Secondly the scope of the "Wertekosmos" remains unclarified with regard to problems of valuation in purely theoretical items. It is a very important problem, I think, to know whether,

for instance, our valuing the books of other philosophers is really justified, or whether any theory exceeds all possible valuation of its goodness. And I find it questionable whether the nature of our 'soul' is not much too dynamic, too diabolical, to be imprisoned in such a cob-web of values, however heavenly in its presentation to us.

HEINZ-JUERGEN SCHUERING

*The Christian Philosophy of Saint Augustine* By ETIENNE GILSON  
Victor Gollancz Ltd, 1961 42s

THE reduction to rigid and systematic order of the philosophical tenets embodied in the multiple and many-coloured works of St Augustine of Hippo is a tough, if not impossible, task. This volume, with its definitive-sounding title and neat chapter-headings, conveys the promise of such a reduction, yet it turns out that the author's original intention was rather more modest. This work is in fact the British reprint of the American translation of *Introduction à l'étude de saint Augustin* (2nd edition, 1943), the preface to which made clear the tentative nature of the enterprise, it was then only suggested that certain dominating "*thèses capitales*" could be usefully brought out; now, however, the new English foreword suggests that there is a coherent system of doctrine within which *any particular point* made by Augustine can be precisely situated. The unfeasibility of this is in fact made plain in the excellent final chapter of the book, wherein the personal, occasional, digressive, activist, and cosmological facets of Augustine's thought are properly stressed. Provided that these are borne in mind, there seems to be no reason why the systematic framework of the exposition should not be acceptable as a useful device in the task of providing what its author calls a "sample map" for beginners, as such, there can be no doubt as to its value.

Whatever one's view on the sense of the term "Christian Philosophy" (and Gilson debates this extensively) the indubitable influence of Augustine on modern philosophy, as well as on medieval, means that he cannot be exiled from departments of philosophy. Many of these pages read like the record of an argument between Descartes and Hume. In general, Gilson's comments on the debt of later philosophy to Augustine are most judicious and useful, although the danger of reading back into Augustine theses of later thinkers is not altogether avoided, in spite of the author's own useful and frequent warnings. Owing to the manner of exposition adopted it is not always clear whether the voice which speaks at any particular point is that of Augustine or that of Gilson. It is, for example, startling to be told that "Some items of rational knowledge are remarkable in that they are truths"; here it turns out that "truth" is being used in an Augustinian fashion, to mean 'necessary truth'. This sort of defect is happily counterbalanced by a massive underpinning of notes containing apt and copious quotations from the Latin, and it is certainly to these and their prolongations in the original works that the serious student will be usefully guided by the present study.

Some, but not all, of the Latin tags and quotations have been dropped from this translation, a translation which is, generally speaking, competent, but which contains lapses. For instance, a British-trained philosopher would look twice before allowing himself to record (p. 33) that Augustine had an obvious tautology as the issue which faced him, and resolved the issue by affirming the truth of that tautology ("Whether reason can or

cannot attain certain truths . . . is a question Augustine would answer in the affirmative"); in actual fact Gilson's text had "*par l'affirmatif*", thereby indicating the affirmative member of the disjunction, and this makes a world of difference. Again, slackness of translation in note 66 (p. 320) makes it look as though Gilson speaks of a "lack of theological competence which is so necessary" when dealing with the problem of grace and free-will. The final reference in note 25 (p. 254) should read "*PL* 32, 1252-53."

D. P. HENRY

*Inductive Probability* By JOHN PATRICK DAY. Routledge & Kegan Paul, London, 1961 (International Library of Philosophy and Scientific Method.) Pp xvi + 356. 40s.

THE book begins with a chapter on the philosophical problem of inductive probability. The chapter is divided into two main parts. The first is concerned with what the author calls 'Attribute-inductions, Variable-inductions and Evidential-statements'; the second part is on 'Probability-judgements, Probabilification-judgements and Evidential-evaluations'. The second chapter is entitled 'Subject-Predicate Inductive Probabilification-Judgement-Formulas'. The third chapter has the heading, 'Functional Inductive Probabilification-Judgement-Formulas'. Then follow chapters on 'Other Alleged Determinants of Inductive Probability', 'Inductive Probabilification by Elimination' and, finally, 'Inductive Reasoning and Inductive Logic'.

By 'inductive probability' the author means the probability of inductions, and an 'induction' is a generalisation or a proposition derived from a generalisation. A generalisation, in its turn, must be explained, according to the author, in terms of the notion of an evidential-statement. A categorical evidential-statement-formula is 'The fact that  $q$  is evidence that  $p$ ' and symbolized by  $E(p, q)$ . The author says that 'it is convenient to call  $q$  the evidencing-statement-formula and  $p$  the evidenced-statement-formula'.

A probability-judgement is 'It is probable that all BU are H'. A categorical probabilification-judgement is 'The fact that all observed BU are H and that they are varied and numerous makes it probable that all BU are H'. This seems to be no more than a probability-judgement in which the evidence is mentioned explicitly. What, then, is an induction? We find, 'By a primitive inductive probabilification-judgement I mean a judgement of which the probabilified-proposition is an induction and the probabilifying-proposition a description'.

I think enough has been quoted here to show that one needs super-human strength to wade through this morass of verbiage. As far as I could discover the author never clearly states even what his particular view of induction is, though he bravely accuses Hempel, Carnap and Popper of confusion. I do not deny that technical terms are needed in order to discuss induction and probability. Probability is, after all, a mathematical concept, and there exists a calculus that can be made use of. I do not deny that new technical terms may have to be coined, provided they are explained properly. I do deny, however, that meaningless jargon is a substitute for technical language. Hyphenation is not enough.

This is the first volume in the New Series of the International Library, edited by A. J. Ayer. It is not an auspicious beginning.

ERNEST H. HUTTEN

*The Quest for Being.* By SIDNEY HOOK. Macmillan & Co. Ltd., 1961. Pp. ix + 254. 45s.

In this book Professor Hook reprints thirteen essays which he wrote between 1934 and 1960. As he says, they may be read in any order, for they do not present any cumulative argument nor betray any notable development of thought. The opening essay discusses the influence of philosophical theories on conduct. It is followed by a piece which argues that moral responsibility may be reconciled with determinism if we can overcome the difficulties of the concept of *blame*. The first part of the book ends with a defence of Dewey's naturalism against the criticisms of Stevenson and others.

The essays in the second part of the book are given unity by their common animus against Christianity. Several of them read like an inversion of the *De Civitate Dei*. As Augustine strove to show that the fall of Rome was not due to the adoption of the Christian religion, so Professor Hook is anxious to prove that the horrors of the second world war were not due to its abandonment. In a paper entitled *Modern Knowledge and the Concept of God* he presents philosophical arguments against theories about God held by Copleston, Einstein, Bergson and others. He concludes this section with a contrast between the existentialism of Feuerbach and that of Kierkegaard.

The attack on Christianity is continued, less frontally, in the essays of the third and last section. One paper analyses and criticises the use of the word "Being" by Tillich, Heidegger, and some neo-scholastics. Another defends the thesis that there is no philosophical knowledge independent of science. A third offers a brisk account of the issues in dispute in the conflict between materialism and idealism. The remaining two outline the naturalistic humanism which is the author's chosen philosophy, and discuss its relation to spiritual values and norms of rational behaviour.

The book has the defects of a collection of reprints. The wartime pieces are now dated, and several of the essays repeat each other. Twice we are told that Christians do not pray to "our nephew in heaven" (pp. 118, 153), twice we are given a quotation from Troeltsch to the effect that the Christian ethos recognises differences in social status as established by the will of God (pp. 15, 101). The level of the discussion varies greatly. Much of the book is journalistic polemic against "the tom-tom of theology and the bagpipes of transcendental metaphysics". It is vivid and vehement without being particularly profound, as the following example will show. "It has sometimes been urged as a mitigating feature of the hierarchical structure of the Church that 'a peasant might become a Pope'. True, but so can an Austrian house-painter or the son of a Georgian cobbler become a Dictator. Does that alter the character of totalitarianism?" (p. 93).

When he moves to more philosophical topics, Professor Hook does very poorly. A single paragraph on page 117, for example, contains four serious errors. Arguing that the notion of *God* is incoherent, Hook takes "God necessarily exists" to mean "'God exists' is a necessary proposition". This is an error. The scholastics who first spoke of God as a necessary being meant merely that He was imperishable. Again, Hook says that judgements based on experience can never be more than probable. This is untrue. Elsewhere, Hook himself recognises that there are many "massive facts of human experience" which we *know*. among his examples

are "tears are usually a sign of grief" and "there are many kinds of processes in the world". It was propositions of just this kind ("some things in the world change") which were the premisses of the traditional proofs of God's existence. Again, Hook says that outside logic there are no necessary truths concerning existence. If, as the context suggests, he is using the word "logic" strictly, then he is mistaken. The proposition "there is a prime number greater than a million" is necessarily true. Finally, Hook says that believers cannot accept any method of argument which leads to the conclusion that there are necessary beings other than God. This is false. Medieval scholastics believed that spirits and human souls were necessary beings.

Professor Hook is correct in thinking that neo-scholastics often talk nonsense about Being. He is wrong in thinking that what leads them to this nonsense is their belief in God (p. 154). Mistakes similar to the scholastic ones can be found in Russell's *Principles of Mathematics*: as Hook knows (p. 150), it was the theory of descriptions that made it easy to see through confusion of this kind. The confusion arises ultimately from carelessness in distinguishing signs from things signified—a carelessness which Hook exhibits more than once. (See, for example, this sentence on page 157: "existence is not a predicate, or attribute, or property like triangular or human".)

Again, there is much in scholastic talk of analogy which is open to criticism. But Professor Hook would have done well to inform himself more thoroughly about it before attacking it. He thinks that analogy of proportion is the same as analogy of proportionality; and he quite fails to see the difference between analogical predication and metaphor. To see this difference, consider the word "good" which, for scholastics, is always an analogical predicate. By this they mean that when I call a penknife "good" and when I call a pancake "good" there is no simple property which I attribute to both of them. But in calling a penknife "good" I am not applying to it a metaphor drawn from pancakes.

Similar evidences of superficiality could be pointed out in many places in *The Quest for Being*. More damaging to the cause of Christianity than any of Hook's analysis or polemic is a defence of religious belief contributed by Ernest van den Haag. Dr. van den Haag believes that religious beliefs are logically quite unjustifiable, but thinks that they should be encouraged as a sedative. Religious sanctions, he writes, are necessary for a society if it is to be stable without being totalitarian. "It is socially and politically not important whether religious doctrines are true. It matters only that they are believed." In the face of such support, the Christian can only echo Professor Hook's indignant rejoinder: *non talis auxilio nec defensoribus istis*.

ANTHONY KENNY

Jean-Paul Sartre—the existentialist ethic NORMAN N. GREENE The University of Michigan Press Pp vii + 213

In this book Mr. Greene sees Sartre as a target for attack from three sides, the side of Catholicism, of Marxism and of Liberalism. His very creditable intention is to understand Sartre first, and only then to allow the question to be raised of whether he is able to stand up to these varying assaults. The purpose of the book, therefore, is primarily expository. But very little

below the surface is an apologetic, or at least a defensive, purpose, and thus obtrudes from time to time in spite of the author's good intentions. Moreover the direction of the attacks has dictated the order of presentation of Sartre's views, which seems somewhat arbitrary to one who is interested in Sartre rather than in what hostile critics have said of him. There is also a tendency to collect defences from any available quarter, and therefore the chronological order of Sartre's work is almost entirely overlooked. In some cases this does not matter, but very often it does. For instance, it is annoying to be told that his views on freedom are *best set out* in some early writing, when it is known that they have later undergone a considerable expansion and modification. It is obviously difficult to give a satisfactory account of the work of a philosopher who is still active, but orderliness, if not completeness, could have been better served here if exposition had been still more ruthlessly divorced from an interest in what the critics had said.

The best part of the book is the treatment of Sartre's political opinions, perhaps because the enemy here, Liberalism, is more indefinite and therefore less obtrusive. In any case it is useful to have some of Sartre's statements about political theory collected together, since they are more widely dispersed in the essays and occasional writings than are his views on epistemology, psychology and ontology. But this part of the book, as well as the rest of it, suffers from the unfortunate accident, for which Mr. Greene is in no way to blame, of having been written before the appearance of the *Critique de la Raison Dialectique*, in 1960. In this enormous book, only the first part of which is so far published, Sartre attempts to unite the existentialist insistence on the individual's freedom of choice and responsibility for his own life, with the Marxist concepts of determinism and scientific history. In a pseudo-Kantian spirit, he seeks to make both Marxism and Existentialism scientific. Anyone might falter before embarking on the exposition of this flamboyantly unreadable monster of a book; but unfortunately Mr. Greene has been in no position even to falter. So his remarks on Sartre's relation to Marx and on his social and political theory, though interesting enough, must be taken as merely introductory.

MARY WARNOCK

*The Correspondence of Isaac Newton.* Edited by H. W. TURNBULL, F.R.S. Volumes I (1661-1675) II (1676-1687) and III (1688-1694) Cambridge. Published for the Royal Society at the University Press. Pp 478, 552 and 445. Price £7 7s each.

THESE two volumes are the beginning of a complete edition of the works of Sir Isaac Newton, undertaken at long last by the society, over which he presided for no less than twenty-three years. They are a fitting monument not only to the genius of their chief author, but to the brilliant mathematical scholarship of their editor, who devoted the closing years of his life to this labour of love. Newton was a genius with an infinite capacity for taking pains, and he exploited that capacity to the full in his youth. At the age of twenty-three, he calculated the area of the rectangular hyperbola to the fifty-second decimal place. This demon of exactitude made him an inept teacher, prevented him publishing his own scientific discoveries, and disqualified him for even the limited social life of a Cambridge college. It is little wonder that in middle life he exchanged the duties of a don for those

of a government official, a significant descent onto the money-grubbing plane of the age into which he had lived. Such was not the plane on which his ancestors lived, when they sowed popular religion in the form of the Authorised Version of the Scriptures, and reaped the harvest of a Puritan Revolution. What Cromwell and his contemporaries sowed may be judged from the works of Locke, Wren, Newton and Bentley, which far surpass anything that the people of these isles has achieved before or since. In these letters, we see the ascetic vigour begotten of these years of national abstinence extending its powers into the realm of abstract thought, and capturing the heights of the mathematical and physical universe before it was fully aware of the magnitude of its achievement. If Newton had never written the *Principia*, his work on the Binomial Theorem, the calculus, and optics would have earned him a unique place in the history of science, but the *Principia* has earned him also a unique place in history, for it is generally regarded as the summit of human intellectual achievement by reason of its originality, logical rigour, and objectivity. Recent research has shown that this masterpiece was an almost accidental consequence of his habit of endlessly revising his mathematical ideas and these letters show that he kept on revising them long after they had appeared in the *Principia*.

The philosophical significance of Newton lies, not in his own contributions to philosophy, but in the implications of his scientific work. He wrote no philosophy, though he had almost all the qualities that are needful for a great philosopher—originality, love of truth and the capacity to express himself clearly and consistently. The explanation of this enigma will be found in these pages. Newton's rigorous empiricism was married indissolubly to a belief in the authority of the Scriptures. He speculated boldly on matters physical and on the details of revealed truth; but he drew a strict distinction between what had been established by scientific method and Christian tradition, and what still lay beyond the reach of experience. Though his speculations on chronology are now of little value, they contain many acute deductions from the premises available to him. His exposure of the unsoundness of the Byzantine and later Latin textual traditions was fully confirmed by the work of Westcott and Hort. His belief that divine intervention was necessary for the creation and maintenance of the solar system provoked the scorn of Leibniz, whose own Pre-established Harmony was pronounced by Newton to be the most improbable of miracles. On questions of ontology, Newton was, of course, a complete amateur compared with Leibniz, for the very good reason that he had no belief in the capacity of abstract thought to discover truth in the absence of explicit data. His natural objectivity made him almost incapable of psychological analysis. Locke's 'new way of ideas' struck no answering chord in his naïve consciousness, though he made one significant contribution to the physiology of binocular vision. Though the correspondence with Bentley in Volume III contains valuable discussions on infinity and equilibrium, it was left to Kant, in the end, to elucidate the philosophical implications of the Newtonian system, nearly a hundred years after the publication of the *Principia*, when the dogmatic idealism of Leibniz was almost an article of faith in the universities of Germany.

Subsequent philosophers have fathered on Newton a system of absolute space, absolute time and instantaneous action at a distance, and have used the discoveries of modern physics as a stick with which to beat the Newtonian philosophy. In fact, their arguments are based on a very superficial study of Newton's views. Newton was always ready to



incorporate new facts into his physical system. His speculations on the more refined forms of matter necessary to explain the phenomena of light, magnetism and gravitation would have enabled him to explain the curvature of the path of light in magnetic and gravitational fields with the greatest of ease. The list of queries at the end of his *Optics*, many of which go as far back as 1675, is a mine of acute speculations on problems that are still awaiting solution. No one was more acutely aware of the differences between the macroscopic and microscopic properties of matter, or of the vast hiatus that still remained to be filled by future research. Other people may have been blinded to new facts by the range and consistency of the *Principia*, but its author certainly was not. The tragedy is that his inability as a teacher and the lack of good scientific teaching in the English schools and universities prevented his work bearing its proper fruit in his own country. He had no notable pupils, and no notable disciples in Great Britain, save David Gregory, Brook Taylor and Colin Maclaurin. It was the Bernoullis, Euler, Lagrange and Laplace, who reaped the harvest that Newton had sown, while Newton himself retired to the Munt, and the best brains of Britain devoted their energies to exposing the supposed villainy of a German philosopher, who, they firmly believed, had filched the secrets of the calculus from the letters of their national hero. It took more than a century for British mathematics to recover from this orgy of jingoism. Great as were the contributions of Locke, Berkeley and Hume to the history of ideas, they only served to emphasise the sterility of the great English universities throughout the eighteenth century.

ARTHUR THOMSON

*Imagination.* By E. J. FURLONG. London: Allen and Unwin (The Murhead Library of Philosophy), 1961. Pp. 125 18s

PROFESSOR FURLONG divides imagination into three leading concepts: *in imagination*, *supposal* and *with imagination*. His notes on these concepts are supplemented by a consideration of the treatment of imagination in Hume and Kant. Two chapters on dreams form the part of the book in which argument is most sustained.

Furlong is concerned to defend the view that dreams are experiences which we have while asleep. He urges, against Malcolm, that when a child on waking tells us that a robber has been chasing him the child has a "conviction that something frightening happened to him while asleep" and this is something we should no more distrust than we should distrust his "memory-convictions" elsewhere. "In so far as a child's memory counts as evidence for the occurrence of a waking event, so also, unless some good reason to the contrary is forthcoming, it counts as evidence . . . for the occurrence of sleeping events. Hence Malcolm's verification requirement is, in fact, supplied" (pp. 46-47).

The difficulty is that the child's conviction that something frightening happened to him goes along with and forms part of his conviction that he was chased by a robber. But the child was *not* chased by a robber. He was in bed all the time: thus his conviction about being chased is a conviction that something which did not happen happened. So if you call this a "memory-conviction" you have to say that the child's memory is here at fault. The child was chased by a robber, not in actuality but in a dream; similarly he was frightened, not in actuality but in a dream, for he was only frightened insofar as he was chased. As something *dreamed*, the memory

of neither would be questioned. As something that actually happened (happened to the child while he was asleep), the only kind of memory from which either gets support is a faulty memory.

According to Furlong, "Malcolm wants us to believe . . . that when we look at the sleeping child and say, 'He must be having a pleasant dream, we are talking nonsense' (p. 46—in this example the sleeping child smiles, and when he wakes up he may tell us he was on a coral island). But Malcolm does not say or imply that we should be talking nonsense in such a case. He maintains there would be an unclarity as to whether we were using the smile as our *criterion* that the child is dreaming or as *evidence* that he will be able to relate a dream; and therefore our words would have no clear sense. According to Malcolm, a concept of dreaming in which the criterion is behaviour is a possible concept, but a secondary concept. He states that "dreaming that has a purely behavioural criterion is of little interest" and that "our primary concept of dreaming has for its criterion, not the behaviour of a sleeping person but his subsequent testimony" (*Dreaming*, p. 63).

R. F. HOLLAND

*The Idea of Order: Contributions to a Philosophy of Politics* By HANS BARTH. D Reidel Publishing Co., Dordrecht, Holland. 1960. Pp. vii + 209. Hfl 15.75

ALTHOUGH the main title suggests a systematic treatise, this book in fact consists of six studies in by-ways of the history of political theory, topped and tailed by two more general essays. The first essay links philosophy and politics through the notion of *Process*, understood chiefly in the sense of a trial. The last essay analyses a 'logic of order' consisting in the three concepts of 'spiritual unity' (or consensus), sanction, and authority, and points out that some political philosophers have given undue attention to one of these three at the expense of the other two. The historical studies share some common ground with this theme, since several of them deal with theorists (Burke, Rivarol, Lamennais, Comte, Maistre) who advocated religious or quasi-religious authority in society, while one sets out the views of Bentham and Proudhon on sanction. These chapters are largely expository, and such criticism as they contain rests on assertion more than argument. Chapter V has an interesting discussion of the influence of de Maistre on Comte.

Hans Barth is Professor of Philosophy at the University of Zurich. The book was originally published in German, and the translators have at times deliberately sacrificed idiomatic English in the interests of a close literal rendering.

D. D. RAPHAEL

*The Democratic Intellect: Scotland and her Universities in the Nineteenth Century* By GEORGE ELDER DAVID. D Litt. Edinburgh: The University Press. 1961. Pp. xx + 352. 50s.

THIS book is a most valuable history of the decline and fall of the Scottish Common Sense Philosophy as a system of higher education. That philosophy had its roots in the forthright intellectualist character of the Scottish people, but it did not become a theory, until the Scottish Newtonians attempted to defend the naive realism of the new physics from the criticisms of Berkeley and Hume. At that time, every Scottish arts student had

to learn the elements of geometry as a system grounded upon indubitable sense-born intuitions, the elements of rhetoric—as exemplified by the simpler classics, Buchanan's versions of the *Psalms*, and a few of the best English authors—and the elements of physics, metaphysics and ethics. The aim of the curriculum was to produce men, who could converse, speak and write sensibly about most topics that were likely to crop up in public life or the professions. It was a system that produced a considerable number of argumentative asses, who usually settled down, like their less opinionated contemporaries, into sensible, upright men of unshakable mental equilibrium. It reached its apotheosis in Sir William Hamilton—a man of vast erudition, sober judgment, and earnest patriotism.

In the first half of the nineteenth century, the increasing wealth and complexity of English society gave rise to a greater need for specialisation, and the movement towards specialisation quickly gained a foothold in the English universities. The younger Cambridge mathematicians discarded the elaborate geometrical methods of the Newtonians, and embraced the more powerful, analytic techniques, that had been developed on the Continent. The disciples of Porson expected their pupils to write elegant Latin and Greek, and to translate and annotate the most difficult classical texts. The scandals of industrialisation raised up a new type of Evangelical enthusiast, like Wilberforce, Shaftesbury and Chalmers, who concentrated on social reform, and had little or no belief in the academic deliverances of Common Sense. In zeal for reform, they were matched by the Utilitarians, who reduced the mind to a calculating machine, and social purpose to the pursuit of pleasure—doctrines that were utterly repugnant to the Faculty Psychology.

The citadel of Common Sense was betrayed from within. Professor Forbes introduced a Cambridge mathematician into the Edinburgh chair, because he wanted to raise the standard of instruction in his physics classes. English professors of Greek in Glasgow and brilliant Snell exhibitioners from Balliol submerged the classical traditions of the land of Buchanan, Johnston, Ruddiman and Melvin. The fathers of the Disruption placed their own Evangelical nominees in the Edinburgh chairs of philosophy. The Commission of 1858 led to the introduction of honours degrees, but not the destruction of the tripartite system. The Commission of 1883 brought about the abolition of the tripartite system for honours students. Then, at last, the best Scottish students could compete with some hope of success for places in the higher civil service, and the Scottish universities became the aristocrats of the red-brick fraternity.

Those who reflect on this drama of the schools may well reach the conclusion that what the Common Sense School was groping after was not a criterion of truth, but a model for the better use of our natural capacities. The faculty model does, in fact, provide a much better schema than the pictures and wax impressions of Locke, and, now that neurology and computer science have brought us within sight of working models, we can see that the true heirs of the Common Sense School were the fathers of modern physiology, not the pious Fraser, nor the pugnacious Ferrier. Fraser was a philosophical nonentity, and Ferrier, separated from Hamilton, never fulfilled the rich promise that he showed in his *Philosophy of Consciousness*. The great merit of Dr. Davie's book is that it makes the social implications of these ancient controversies so obvious and exciting. In the opinion of the reviewer, he has written one of the most interesting books on the history of ideas that has appeared in recent years.

ARTHUR THOMSON

Received also —

- J Bailhe, *The Sense of the Presence of God*, Oxford University Press, 1962, pp ix + 269, £1 10s
- E W Beth, *Formal Methods*, Dordrecht, D Reidel Publishing Co, 1962, pp xiv + 170, f 23 50
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